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Vol. XV, No. 1 · July-September 1974

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1 Darnaway Street, Edinburgh, EH3 6DW, Scotland

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VOL. XV, NO. 1 JULY-SEPTEMBER 1974

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Natural Selection Through Slavery and the African Slave Trade

By NATHANIEL WEYL

While contemporary studies of the American Negro and the African slave trade are voluminous, they concentrate primarily on sociological, ideological and moral aspects of these institutions. They cast little light on the genetic implications for the Negro population of the New World of the slave trade and chattel slavery.

An exception is a recent book on comparative Negro-White intelligence by Dr H. J. Eysenck, which contains these speculations:[1]

> If, for instance, the brighter members of the West African tribes which suffered the depredations of the slavers had managed to use their higher intelligence to escape, so that it was mostly the duller ones who got caught, then the gene pool of the slaves brought to America would have been depleted of many high-I.Q. genes. Alternatively, many slaves appear to have been sold by their tribal chiefs; these chiefs might have got rid of their less intelligent followers. And as far as natural selection after the shipment to America is concerned, it is quite possible that the more intelligent Negroes would have contributed an undue proportion of "uppity" slaves, as well as being much more likely to try to escape. The terrible fate of slaves falling into either of these categories is only too well known. White slavers wanted dull beasts of burden, ready to work themselves to death in the plantations, and under these conditions intelligence would have been counter-selective. Thus there is every reason to expect that the particular sub-sample of the Negro race which is constituted of American Negroes is not an unselected sample of Negroes, but has been selected throughout history according to criteria which would put the highly intelligent at a disadvantage. The inevitable outcome of such selection would of course be the creation of a gene pool lacking some of the genes making for higher intelligence.

These aprioristic speculations inevitably over-simplify a rather complex situation. Dr Eysenck is, I believe, on sounder ground when he discusses the selective influences of the African slave trade than when he expatiates on those of plantation slavery. His harsh judgments in the latter area are perhaps more realistically

[1] H. J. Eysenck, *The I.Q. Argument: Race, Intelligence and Education*, Library Press, New York, 1971, p. 42.

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descriptive of sugar-plantation slavery in such areas as the British West Indies, Surinam and Brazil than they are of the cotton and general farming slavery of the Southern United States.

The I.Q.-test scores of native Africans are generally much lower than those of U.S. Blacks.[2] This does not, however, necessarily refute Eysenck's hypothesis that the latter are genetically less intelligent than their African populations of origin. American Negroes have been exposed for generations to modern education, urbanization and industrialization. Moreover, they are a racially mixed population, with perhaps 16 per cent white genes.[3]

Who Were Chosen?

Abolitionist propagandists gave wildly inflated estimates of the total number of Africans sent as slaves to the Americas. Thus, R. I. Rotberg cites the eminent demographer, Robert R. Kuczynski, as authority for the alleged fact that "at least twenty-million slaves" were victims of the trade.[4] Basil Davidson, the author of books which make extravagant claims for the glories of extinct Negro civilizations in Africa, also cites Kuczynski, but quotes him correctly and gives fifteen millions as his estimate.[5] However, the late Dr Kuczynski did not arrive at his estimate by a process of demographic analysis, but borrowed his round figure from an obscure propagandist of the 1860s named Edward E. Dunbar who published polemical articles against slavery in order to drum up American support for the cause of Benito Juarez in Mexico.[6]

[2] L. E. Andor, *Aptitudes and Abilities of the Black Man in Sub-Saharan Africa,* 1784-1963: *An Annotated Bibliography,* National Institute for Personnel Research, South African Council for Scientific and Industrial Research, Johannesburg, 1966. This bibliography comprises 486 items on intelligence and 35 on personality.

[3] Nathaniel Weyl, "Some Genetic Aspects of Plantation Slavery," *Perspectives in Biology and Medicine,* Summer 1970, pp. 618-625. The article points out that prevalent estimates that the American Negro is 25 per cent Caucasian are based on serological data from Baltimore and other large cities which selectively attract light-skinned and racially-mixed Blacks. A study of rural Georgia Negroes, by contrast, shows only 9 per cent white genes. In each case, race mixture is determined by the R° chromosome combination in the Rhesus blood group, or cDe in the R. A. Fisher notation.

EDITOR: I have argued elsewhere that the blood groups do not necessarily show the amount of admixture. Therefore, I think the Caucasoid element in the American Negro may be higher.

[4] Robert I. Rotberg, *A Political History of Tropical Africa,* New York, 1965, p. 152.

[5] Basil Davidson, *Black Mother,* London, 1961, p. 89.

[6] Edward E. Dunbar, "History of the Rise and Decline of Commercial Slavery in America, with Reference to the Future of Mexico," *Mexican Papers* 1, No. 5, April 1861, pp. 269-270.

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Fortunately, Professor Philip D. Curtin has published a study of the African slave trade which scholars recognize as definitive. In it, he reaches the conclusion that an estimated 9,566,100 Africans were transported as slaves between 1451 and 1870 and that, of these, all but 175,000 were shipped to the Americas.[7]

Estimates of the number of slaves imported into the United States from the Colonial era to the Civil War also vary wildly. Interestingly enough, the contemporary estimates were considerably more accurate than those of some modern authorities. Thus, in 1853, the pioneer statistician, Henry C. Carey, estimated that from 250,000 to 300,000 slaves had been brought into the country.[8] These modest figures contrasted with the assertion of Noel Deer, the historian of the sugar industry, that 1,500,000 Africans had been brought in prior to the outlawry of the trade in 1808 and another million thereafter.[9]

Curtin, who reviewed all these estimates in detail, came to the conclusion that only 399,000 Africans were brought into the United States during the four centuries of the slave trade.[10]

Almost half of the slaves brought into the United States came from the Bight of Biafra and from Angola, according to Curtin. An estimated 15.9 per cent came from the Gold Coast, 13.3 per cent from Senegambia and another 11.4 per cent from the Windward coast. South Carolina slave-owners had strong views about the comparative value of the different African tribes. They gave first preference to Negroes from Senegambia, gave the Gold Coast Blacks second choice, and sought to avoid those from the Bight of Biafra.[11]

" Persons became slaves," Ulrich B. Phillips wrote in his great classic on American slavery, " through capture, debt or malfeasance, or through the inheritance of the status."[12]

[7] Philip D. Curtin, *The Atlantic Slave Trade,* University of Wisconsin Press, Madison, 1969, p. 269, Table 77.

[8] Henry C. Carey, *The Slave Trade, Domestic and Foreign,* Philadelphia, 1872, p. 18. The first edition appeared in 1853.

[9] Noel Deer, *The History of Sugar,* London, 1949-1950, Vol. 2, p. 282.

[10] Philip D. Curtin, *op. cit.,* p. 269.

[11] Philip D. Curtin, *op cit.,* p. 157.

[12] Ulrich B. Phillips, *American Negro Slavery,* Louisiana State University Press, Baton Rouge, 1966, p. 6. This pro-Southern account of the peculiar institution was first published in 1918. Hailed at the time as a classic, it has been treated " with hostility, suspicion and even contempt " by modern historians who object to its viewpoint. Yet, in a measured introduction to the 1966 edition, Professor Eugene D. Genovese, a Marxist authority on slavery, calls attention to the excellence of Phillips's research and the penetrating character of some of his insights. " Phillips," Genovese writes, " came closer to greatness as historian, perhaps as close as any historian this country has yet produced." (p. vii)

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In the Gold Coast, the Fanti acquired slaves either by waging war on other tribes or by trade with the dreaded Ashanti. The latter "obtained their captives either in upcountry wars or in trade with the neighboring savanna tribes farther north, and in later years, they may also have bought a few petty criminals and Bantu prisoners from the Hausa traders who traversed the interior."[13]

In general, we can conclude that the slaves destined for the Americas had several characteristics not necessarily shared by the majority of the black populations from which they derived. Obviously, they were drawn primarily from tribes which were unsuccessful in warfare, from the weak, defeated and subjugated. Where they were obtained by means of intertribal trade, they would consist disproportionately of people whom the chiefs wanted to get rid of. This would include criminals and other offenders, incompetent workers, the inept, the unruly and the slow-witted. Finally, they had to survive the ordeal of transportation over enormous distances from their upland points of capture to the seaboard trading stations where they were sold to European purchasers. Phillips[14] writes:

> The coffles came from distances ranging to a thousand miles or more, on rivers and paths whose shore ends the European traders could see but did not find inviting. These paths, always of single-file narrowness, tortuously winding to avoid fallen trees and bad ground, never straightened even when obstructions had rotted and gone, branching and crossing in endless network, penetrating jungles and high-grass prairies, passing villages that were and villages that had been, skirting the lairs of savage beasts and the haunts of cannibal men, beset with drought and famine, storm and flood, were threaded only by negroes bearing arms or bearing burdens. Many of the slaves fell exhausted on the paths and were cut out of the coffles to die. The survivors were sorted by the purchasers on the coast into the fit and the unfit, the latter to live in local slavery or to meet either violent or lingering deaths, the former to be taken shackled on board the strange vessels of the strange white men and to be carried to an unknown fate.

When he explored its hinterland in 1795-1797, Mungo Park estimated that the slave population of the Niger Valley outnumbered the free by three to one. The hereditary slave class was the source of manpower for sale to the Europeans. As a rule, household slaves would not be sold for ocean transport since

[13] Donald L. Wiedner, *A History of Africa South of the Sahara,* Vintage Books, New York, 1964, p. 62.

[14] Ulrich B. Phillips, *op. cit.,* p. 31.

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that would induce their fellows to run away. But when they were captured in war by a rival tribe, these domestic slaves would be sent in coffles to the trading factories.

In addition to the winnowing effect of surviving the long march in coffles from upland to seaboard trading post, the slaves faced the selective process of the Mid-Passage. The former eliminated the physically weak and diseased; the main hazards of the latter were dysentery, scurvy and smallpox.

The horrors of the Mid-Passage have been inflated by abolitionist writers. Moreover, risks and discomforts which seem unbearable to modern readers, used to ocean-liner or jet-plane travel, would have appeared normal to free men and women who travelled steerage at the time. Consider the testimony of Henry Laurens, "an atypical South Carolinian for he loathed slavery and had no qualms about the mass arming of Negroes" in the American Revolutionary War.[15]

Laurens withdrew from the slave-trading business in 1768 on moral grounds. He wrote:[16]

> Yet I never saw an instance of cruelty in ten or twelve years' experience in that branch equal to the cruelty exercised upon those poor Irish . . . Self-interest prompted the baptized heathen to take some good care of their wretched slaves for a market, but no other care was taken of those poor Protestant Christians from Ireland but to deliver as many as possible alive on shoar upon the cheapest terms, no matter how they fared upon the voyage nor in what condition they were landed.

William Snelgrave, a veteran captain in the slave trade, customarily told his cargo that they were being taken across the ocean to work the land, not to be eaten; that if they were mistreated, they were to complain to the interpreter; that if they struck a member of the crew, they would be severely punished.

It is also worth noting that the mortality of slaves during the Mid-Passage was substantially lower than that of the crews which transported them. For example, Curtin cites figures for Bristol ships in the 1780s. The death rate per thousand seamen per year was 10 in the Greenland trade, 41 in the East Indian trade, and 219 in the slave trade.[17] The death rate per annum among British

[15] Nathaniel Weyl and William Marina, *American Statesmen on Slavery and the Negro,* Arlington House, New Rochelle, 1971, p. 35.

[16] Quoted in Ulrich B. Phillips, *op. cit.,* p. 36. These victims were Scotch-Irish. Eighty years later, planters in the new cotton lands of Mississippi, Arkansas and Texas would use Irish Catholic laborers rather than Negros, to dig post holes in malaria country infested with rattlesnakes. The death of Irishmen was of no importance, that of Negroes represented a substantial loss of capital.

[17] Philip D. Curtin, *op. cit.,* p. 285.

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military personnel living in the Gold Coast during 1817-36 was 668 per thousand.[18] These excessive death rates were largely due to the fact that Europeans had little immunity to West African diseases. Nevertheless, they also suggest that the trials and perils of the African slaves during their shipment to the Americas were not extraordinary in comparison with those of free white men in the tropics.

Coromanty Negroes, an African Aristocracy?

The market preferences of American purchasers obviously influenced the selective process of the slave trade. Buyers and slave-catchers would concentrate on those regions and tribes which were esteemed as slave labor and, other things being equal, would avoid those held in low repute.

Unfortunately, the evidence concerning these preferences has not been carefully studied by historians. Moreover, the designations are often vague and inaccurate and at times vary from one slave-using area to another. Nor do the tribal designations used by eighteenth century writers correspond to those currently in use.

Eysenck makes the somewhat simple-minded assumption that white slave traders wanted "dull beasts of burden" rather than Negroes capable of acquiring skills. This view is not substantiated by the evidence. Nor is it validated by the opinions which contemporary authorities held concerning the various tribes of West Africa.

Thus, the Coromanty Blacks from the Gold Coast, belonging to the Akan ethnic group, were "frequently instigators of slave conspiracies and insurrections. Yet their spirit of loyalty made them the most highly prized of servants by those who could call it forth."[19] Some eighteenth century writers urged planters to avoid these natural leaders of insurrections; others expatiated on their courage and manly qualities. The Jamaican planters had a very high opinion of these Coromanty Blacks and had much less use for those from the Niger delta. South Carolinians considered them far better material than Blacks from Biafra.

[18] Philip D. Curtin, "Epidemiology and the Slave Trade," *Political Science Quarterly*, Vol. 83, 1968, p. 203.

[19] Ulrich B. Phillips, *op. cit.*, p. 42, quotes a 1701 report to the Board of Trade by Christopher Codrington, Governor of the Leeward Islands. "The Coromates are not only the best and most faithful of your slaves," he wrote, "but they are really all born heroes. There is a difference between them and all other negroes beyond that 'tis possible for your lordships to conceive. There never was a raskal or coward of that nation . . . My father, who had studied the genius and temper of all kinds of negroes forty-five years with a very nice observation, would say, noe man deserved a Corramante that would not treat him like a friend rather than a slave." *Calendar of State Papers, Colonial Series, America and West Indies*, 1701, pp. 720-721.

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In Surinam, or Dutch Guiana, the leaders of the slave revolts were invariably Coromantys. The word derives from the Coromantyne river in the Gold Coast where the Dutch had two slave factories. The Dutch traded directly with the Ashanti, a congery of tribes which developed in the eighteenth century into an aggressive warrior nation that prospered by seizing slaves in raids and battles. Probably, the Africans who fell victim to the powerful Ashanti armies were a more hardy and courageous breed than the criminals, outcasts and slaves by birth offered for sale to the European factories by the less military black slave-catching tribes. In 1740, a war, which was to last more than thirty years, broke out between the Ashanti and the Wassa, in which the prisoners which each side took were sold to the slave factories.[20] The Coromantys were " so warlike, so prone to cause uprisings —that only Dutch and English colonies consented to buy them and in these colonies when slave rebellions broke out to alarm the white planters, they were led by the proud Coromantyne slaves."[21]

Captain John Gabriel Stedman, the chronicler of the slave wars in Surinam, states that the Coromanty Negroes are more " esteemed " than any other Blacks. He adds: " . . . if they think their punishment unmerited, immediate suicide is too often the fatal consequence, especially among the *Coromantyn* Negroes, who frequently, during the act of flagellation, throw back their heads and *swallow their tongues*, which chokes them upon the spot, when they drop dead in the presence of their masters."[22]

Brian Edwards, the distinguished historian of slavery in the Antilles, held similar views concerning the Coromantys and was ambivalent about their relationship to the slave system:

> Firmness of mind and body, and a ferociousness of disposition, distinguish the Koromantyn or Gold Coast negroes from all others: but with these, they possess activity, courage, and a degree of magnanimity which incited them to difficult and dangerous enterprises, and enabled them to meet death in its most terrible shape with either fortitude or contempt. As the Gold Coast too is inhabited by various different tribes which live in a state of perpetual hostility with each other, it is indisputably certain that many of the captives taken in battle

[20] Rudolf van Lier, *Samenleving in een Grensgebied: Een Sociaal-Historische Studie van die Mattschappij in Suriname* (*Living Together in a Frontier Area: a Socio-Historic Study of Surinam Society*), Martinus Nijhoof, The Hague, 1949, pp. 122-123.

[21] Melville J. and Frances S. Herskovits, *Rebel Destiny*, Whittlesey House, New York, 1934, p. 323.

[22] Captain John Gabriel Stedman, *Narrative of an Expedition to Surinam*, J. Johnson, London, second edition 1813, Vol. II, p. 280.

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and sold in the European settlements were free in their native country, and perhaps had slaves in their own possession. It cannot be an object of wonder that such men should endeavour, even by the most desperate means, to regain the freedom which they have lost; nor can any thing further be necessary to excite them to action, than the circumstances of being sold into captivity in a distant country. This was the origin of the negro-rebellion which happened in Jamaica in 1760.

After describing the intrepidity with which the Coromantyn leaders of this rebellion suffered being slowly burned alive and being left to die suspended on gibbets, Edwards[23] continued:

> The fortitude and unconcern, with which these people suffer death, is without doubt in a great measure produced by their national manners, wars and religion, all which are in the highest degree savage and bloody. Every master of slaves on the Gold Coast has power over the lives of his slaves; and this power he exercises on every trifling occasion, without hesitation or remorse. Fathers too have the like power over their children. In their wars they are bloody and cruel, beyond any nation that has ever been known to exist; for they murder, with the most horrible barbarity, all their captives that they do not reserve for slaves; cutting them across the face, and tearing away the under jaw, which they preserve as a trophy, and then leave the unhappy victims to perish in that shocking condition . . .
>
> It is surely to be lamented that a people, who naturally possess such emulation and intrepidity, should be sunk into such a lamentable state of barbarity, and oppressed by the yoke of such rigid slavery. For qualities so admirable as these plainly evince that they are born free, and are only accidentally slaves, since (as the Stagyrite beautifully observes) "those who are born with strong bodily and weak mental powers are born to serve; but whenever the mind predominates over the body, it confers natural freedom on its possessor."

The anonymous "professional planter" who wrote a rule book for slave management in the sugar islands was more realistic about the Coromantys. "The inhabitants of the Gold Coast being habituated from infancy to war, the necessary movements of which give flexibility to the muscles and energy to the mind, are by far the most hardy and robust," he wrote, "yet bringing with them into slavery lofty ideas of independence, they are dangerous

[23] *An Abridgment of Mr Edwards's Civil and Commercial History of the British West Indies,* J. Parsons and J. Bell, London, 1794, Vol. II, pp. 373-379. The quotation is presumably a paraphrase of Aristotle's *Politics,* Book I, Chapter 5, pp. 1254b-1255a.

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inmates on a West India plantation, when implicit subjection must necessarily be exacted."

The planter points out that these Gold Coast Blacks were primarily responsible for the major slave rebellions in Jamaica and were excluded from the Windward Islands despite their "superior aptitude" because of their "mischievous spirit." Wishing "hardy" slaves, the planters of Tobago "purchased largely of the Coromantins or Phantees; in consequence of which they had soon reason to repent of their imprudence by insurrections, which greatly retarded the progress of that colony, and occasioned a considerable effusion of blood."[24]

The distinguished writer and scholar, Robert Graves, who was presumably ignorant of the outstanding ability and courage of the Coromantyn Negroes, nevertheless advanced the interesting hypothesis that they are partially of Caucasian stock. Asserting that the word *Koromantse* derives from the Goddess Ker, who gave her name to the Carians, Graves states that Libyans of Cushite-Berber stock were subjugated in the second century A.D. and driven to the south bank of the Upper Niger where they fused with the native Negro population.[25] Thence, they might have moved westward to lands within the reach of the warlike Ashanti. However, blood-type analysis shows that the people of the Gold Coast are preponderantly Negro. One of the most significant serological indices of negritude is the proportion of R° (or cDe) chromosomes in the Rh group. This has been placed at 48 per cent for the Gold Coast Ewe and 56 per cent for the Ashanti as against 49* per cent for New York Negroes and 3 per cent for American Whites.[26]

The character of Dr Kwame Nkrumah, the late Ghana dictator, may seem inconsistent with the virtues imputed by eighteenth century writers to the Negroes of the Gold Coast. I am reliably informed, however, that one of Nkrumah's best kept secrets was that he was not of Gold Coast tribal origin, but belonged to a Liberian tribe which enjoyed low status because of its hereditary occupational association with the removal of night soil.

[24] *Practical Rules for the Management and Medical Treatment of Negro Slaves in the Sugar Colonies,* by a Professional Planter, London, 1803, part I, chapter I. Reproduced in John R. Commons, *et al.* (editors) *A Documentary History of American Industrial Society,* Arthur H. Clark Company, Cleveland, 1910, Vol. II: *Plantation and Frontier,* pp. 127-128.

[25] Robert Graves, *The Greek Myths,* Braziller, New York, 1957, Vol. I. p. 33.

* Since the American Negro frequently has Caucasoid genes this figure being lower than that for the Ashanti is to be expected.—EDITOR.

[26] A. E. Mourant, *The Distribution of Human Blood Groups,* Blackwell Scientific Publications, Oxford, 1954, Tables 23 and 27.

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Other African Tribes as Slaves

There is some disagreement among eighteenth century writers on slavery as to the characteristics of the different West African tribes and their comparative suitability for plantation labor. Edwards, who wrote with more authority and exerted a wider influence than the other sources I have quoted, deals with the putative ethnopsychology of the black tribes at length.

The Mandingoes (Mande peoples), Edwards tells us, practice circumcision, fast on Fridays, and "have not in common the flat noses and thick lips of the more southern natives; and they are in a great measure free from that strong and fetid odour which exhales from the skin of the latter." A few of them are literate and they consider themselves "far superior to the rest of the slaves." They are distinguished by "the great gentleness of their manners," by the fact that "they are more inclined to theft than any of the African tribes," and by their poor adaptability "for hard labour."[27]

If the Mandingoes were often preferred as slaves in the Thirteen Colonies and in the United States, the Papaws, or people of Whidah, were regarded by Edwards as "beyond all doubt the most tractable slaves that are imported from any part of Africa." While he considered them greater thieves than the Gold Coast Blacks, they "pay the greatest attention to agriculture in their native country" and "dread pain" more than other tribes.

Finally, there are the Eboes from the Bight of Benin, "the lowest and most miserable of all the nations of Africa." Although they were cannibals at home, these Eboes are distinguished by a "meekness and humility of deportment which forms a striking contrast to the bold and fearless temper of the Koromantyn negroes." Yellowish in complexion, they seemed diseased to Edwards even when in perfect health. "The principal objection to the Eboes as slaves is their natural timidity and dejection." He added that their features bear "a great resemblance to that of the baboon.[28]

The anonymous professional planter agreed with Edwards on the superiority of the Papaws as slaves, but he found the Mandingoes "reared in the habits of indolence," with "nerveless frames which easily sink under fatigue . . ." While unfit for field labor, they could be used as watchmen or in the distilleries and sugar boiling houses. The Eboes, he thought "turbulent, stubborn and much addicted to suicide," but "hardy and susceptible of labour . . ." From the kingdom of Gabon, "a good negro was scarcely ever bought." Weak in physique, they were bought cheaply

[27] Edwards, *op. cit.*, pp. 372-373.

[28] Edwards, *op. cit.*, pp. 381-383.

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and sold to planters ignorant of their defects. The professional planter suggested that "the gentlemen who have attached themselves to the abolition of the slave trade would do well to commence their reform by obtaining a law for the suppression of this part of the traffic (the importation of Gabon Negroes), which would injure nobody."[29]

These and other writings indicate a somewhat ambivalent attitude toward their Negro slaves by the Antilles planters. They admired the warlike tribes, in particular the Coromantyn Blacks, and despised the weak and timid ones. The drift of their somewhat confused advice was in favor of preferring Papaws, who struck a mean somewhere between the intrepid and rebellious Coromanty Negroes and the meek and humble Blacks from closer to the Equator.

The Planters' Need for Skilled Labor

It is a fairly safe assumption that those writers who theorize about the planters' desire for a labor force consisting of "dull beasts of burden" have never had the responsibility of managing crews of workmen.

Even the most cursory examination of chattel slavery should have convinced Dr Eysenck that one of its worst aspects from an economic standpoint was the inability or unwillingness of the Blacks to acquire labor skills or apply intelligence to their occupations. Planters had to pay what they considered to be exorbitant wages to free white workers whenever they were faced with repairs or construction requiring a minimum of skill.

Throughout the ante-bellum period, the South failed to develop a viable livestock agriculture, partly due to "the brutal and careless treatment that slaves accorded livestock."[30] Crude and heavy farm implements were used, but even those were habitually broken and left in disrepair. Maintenance of equipment, repairs, household industry and skilled work in general had to be assigned to paid white workers. "The harsh treatment that slaves gave equipment shocked travellers and other contemporaries, and neglect of tools figured prominently among the reasons given for punishing Negroes," Genovese observed.[31] Even in Virginia, which was in advance of the Deep South in agricultural techniques, the slave plantations had to discard the "Yankee hoe," which slaves habitually broke, in favor of the clumsy "nigger hoe," which weighed three times as much.

A basic principle of the slave economy, Karl Marx observed,

[29] Professional Planter, *op. cit.*, pp. 128-130.

[30] Eugene D. Genovese, *The Political Economy of Slavery*, New York, 1965, p. 110.

[31] Eugene D. Genovese, *op. cit.*, p. 55.

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is " only to employ the rudest and heaviest instruments and such as are difficult to damage owing to sheer clumsiness." In the slave states bordering on the Gulf of Mexico down to the date of the Civil War, plows constructed on old Chinese models, which turned up the soil like a hog or a mole, instead of making furrows, were alone to be found. Marx[32] quoted Frederick L. Olmstead's classic, *A Journey in the Back Country* (1860):

> So too when I ask why mules are so universally substituted for horses on the farm, the first reason given, and confessedly the most conclusive one, is that horses cannot bear the treatment that they always must get from the negroes; horses are always soon foundered or crippled by them, while mules will bear cudgelling, or lose a meal or two now and then, and not be materially injured, and they do not take cold or get sick, if neglected or overworked. But I do not need to go further than the window of the room in which I am writing, to see at almost any time, treatment of cattle that would ensure the immediate discharge of the driver by almost any farmer owning them in the North.

Genovese imputes these clumsy, cruel and destructive working habits to the outrage of the Negroes at their enslaved condition. Yet similar lack of skill or forethought of any sort can be seen in the Bahamas, among a black population which has been free for a century. Whether the incompetence of the slaves was due to resentment or to deficiency in intelligence, it was an economic burden that inflated the cost of farming unreasonably. If intelligent tribes had been found in Africa, willing to acquire skills and responsibility, they would have been imported at premium prices. As it was, skilled Negroes always commanded large premia over mere brute muscle on the slave marts. This alone serves to refute Eysenck's economic and psychological assertions.

Genetics of Manumission

I have pointed out elsewhere that differential mortality rates " favored cotton and tobacco workers as against rice and sugar workers, house servants as against plantation hands, craftsmen as against unskilled laborers. This tended to give Negroes with white blood higher rates of natural increase."[33] A somewhat extreme statement of the difference in living conditions and life expectancy between field hands and other slaves comes from Stedman's work on Surinam. Of the 80,000 slaves in the colony, Stedman estimated that 30,000 artisans, house workers and specialized estate workers " live better than the common people of England," that another

[32] Karl Marx, *Das Capital*, Vol. I, pp. 219-220, fnt.

[33] Nathaniel Weyl, *op. cit.*, p. 518.

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30,000 "are kept in idleness and do no work in the fields," and that the remaining 20,000 are "among the most miserable wretches on earth; and are worked, starved, insulted, and flogged to death." These 20,000 field hands had an average life expectancy under slavery, Stedman calculated, of ten years.[34]

The household workers, artisans and specialized estate workers were presumably chosen on the basis of intelligence, skill, physical attractiveness, and willingness to learn and adapt. In other words, they were winnowed out of the slave mass on the basis of brain and degree of white admixture.[35] The field hands were chosen for stamina and strength and because they lacked any redeeming features, in the eyes of their masters, that would have promoted them to more congenial occupations. This process of selection tended to give slaves with white genes lower mortality rates and hence a differential survival advantage.

Ironically, manumission of slaves, which was highly selective for white genes and which was regarded by masters and servants alike, as a signal mark of favor, generally operated in the opposite direction. As I wrote in a recent article:[36]

> Demographic conditions conspired to decrease the rate of population growth among free Negroes far below that among slaves. This condition prevailed in the United States both because the medical care which masters gave their slaves from reasons of self-interest was markedly better than that which the free Negroes obtained for themselves and because the emancipated black population was unwanted by white society, despised, impoverished and socially degraded. In areas such as the West Indies where the mulatto and free Negro populations enjoyed a more stable social status (one superior to that of the slaves), this difference may not have prevailed. If so, the comparative survival rates of free Negroes and black-white crosses would be materially different in the Antilles than was the case in the United States.

[34] Captain John Gabriel Stedman, *op. cit.*, Vol. II, pp. 393, 373.

[35] The admixture of Caucasian genes could have come from contact with Hamites or Arabs in Africa or from miscegenation under slavery. First, the preponderant evidence from I.Q. tests is that intelligence is positively correlated with white genes. Second, most people define physical attractiveness in terms of their own race; if slave-owners were no exception, they would have given preference to racially mixed Negroes. Third, degree of body odor is important in choosing household workers in the tropics; the apocrine glands of Negroes secrete the ensyme, acetylcholinsterase, causing their musky odor, whereas the apocrine glands of Caucasians do not secrete this enzyme, and the Mongoloids have hardly any apocrine glands at all. — Carleton S. Coon, *The Living Races of Man*, Alfred A. Knopf, New York, 1965, p. 14.

[36] Nathaniel Weyl, *op. cit.*, pp. 618-619.

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The statistical evidence in support of the assertion that manumission was dysgenic in the United States is so clear that it is astonishing that scholars have not heretofore stressed it.

Between 1850 and 1860, Census figures show that the slave population increased 28.8 per cent, the free Negro population only 12.3 per cent. These gains were due almost exclusively in both instances to natural increase. Slave imports via the illegal African trade must have been offset by manumissions, escapes and Liberian colonization. The free Negro population probably gained more from manumission than it lost by emigration and passing for white.

Contemporary records in Northern cities with good birth and death statistics reveal a uniform pattern of excessive mortality among free Negroes. In Boston in 1855-1859, Negro deaths exceeded Negro births by almost two to one, in Philadelphia in 1860 by more than two to one. In Rhode Island and Connecticut, Negro and mulatto deaths regularly exceeded births.[37]

In his outstanding study, *The Health of Slaves on Southern Plantations,* William Dosite Postell reviewed the testimony of contemporary physicians that the Negro slave received "good care, wholesome diet, prompt medical attention, and restraint from dissipations that were injurious to his health" and was "healthier in the main than the whites." Tuberculosis, scrofula, cancer and syphilis were described as rare among plantation slaves. By contrast, in the post-bellum period, the Negro tuberculosis mortality rate rose to double that among Whites. Postell's examination of numerous plantation records convinced him that slave mortality was about equal to that of the white people in the same region. The slave infant mortality rate for the plantations studied averaged 152.6 per thousand. By comparison, as late as 1915, the infant mortality rate among Negroes was 163 in Massachusetts, 185 in Pennsylvania and 192 in New York.[38]

Thus, ignorance and poverty combined to drive free Negroes toward biological extinction. This was an unmitigated misfortune for the Negro population of the United States since those slaves manumitted prior to 1863 formed the foundation for a potential aristocracy of intellect. It was a tragedy for a man like Thomas Jefferson who fathered five children by his quadroon slave mistress, Sally Hemings, and manumitted them in his will.[39]

[37] *Introduction to the* 1860 *Census of the United States,* Government Printing Offices, Washington, 1860, pp. 7-8.

[38] William Dosite Postell, *The Health of Slaves on Southern Plantations,* University of Louisiana Press, Baton Rouge, 1951, pp. 143, 158.

[39] For the evidence see Nathaniel Weyl and William Marina, *op. cit.,* pp. 94-99.

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SUMMARY

The African slave trade operated as a selective force in favoring Negroes from defeated or unmartial tribes, Negroes who belonged to hereditary slave classes, Negroes whom their chiefs wished to get rid of because of real or imagined inadequacies or offenses, and Negroes with the physical stamina to survive the long march to the coastal factories and with the resistance to disease needed to live through the Mid-Passage. The selection of African tribes by the slave-owners was based on ethnopsychological evaluations based entirely on subjective impressions and was somewhat confused as to direction and impact. Surveying the process of slave-catching and transportation to the Americas as a whole, however, Eysenck's generalization that the process was selective for lower-than-average intelligence is probably well founded.

Upon their arrival, the Negroes were exposed to winnowing forces of a different nature. Physical strength may have become disadvantageous since the most hardy slaves were assigned to field gangs where mortality was high.[40] Selection for the covetted jobs of household workers and skilled artisans was based on natural intelligence, ability to learn quickly, manual dexterity, physical attractiveness and possession of white genes. If this selective process was positive for intelligence, manumission operated in the contrary direction by casting the most intelligent elements in the Negro population adrift, where poverty, superstition, ignorance of medicine and exposure to the diseases of urban slums took a dreadful toll.

To attempt a quantitative evaluation of all these causal factors and forces to see whether the process was selective for intelligence or the reverse would be a fascinating field of research. Unfortunately, not even a beginning has been made in this task.

[40] Once assigned to the field gang, however, superior physical strength and stamina would be survival advantages.

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The Case of William Shockley

By JULIUS EPSTEIN

Dr William Shockley, professor at Stanford University and a Nobel prize-winner for his contributions to the invention of the transistor, was denied the right to teach a graduate special course on his research in "dysgenics." The denial was communicated by Lincoln Moses, Dean of the Graduate School, Stanford University.

Shockley defines dysgenics as "the study of mechanisms adverse to human genetic quality, particularly retrogressive evolution through disproportionate reproduction of the genetically disadvantaged."

After three months of deliberation, a faculty advisory committee recommended approval provided the class were *not* taught for credit. The committee also stipulated that the course be restricted to one *single* quarter term and be presented only *once*. Dr Moses demurred and made the decision not to authorize the course at all.

In his letter of notification to Professor Shockley, Dr Moses wrote: "Your expertise for teaching this course is subject to doubt."

Shockley's right to lecture about his thoughts and findings is guaranteed under the rules of academic freedom. This freedom is explicitly granted to all members of the Stanford faculty and, of course, to every teacher at any U.S. university. This right is not, and cannot be abrogated by controversy about a teacher's opinions. In fact, academic freedom is designed to protect academics against the hazards of controversy. The denial of Shockley's right to teach was, therefore, most surprising. This infringement raised the question of whether the Dean of the Graduate School, Stanford University, is opposed to academic freedom or whether he does not understand the meaning of this basic right.

Professor Shockley responded to Dean Moses by stating that "the threat to academic freedom was trivial compared to the administration's illusion that all races of mankind are genetically equal." This opinion, he added, reflects the "illusion of flat human quality," which is "so central to the thinking of the Graduate School administration at Stanford as to exclude a sincere search for truth."

Shockley proposed to teach a course on new methods of research dealing with the "determination of the Caucasian fraction

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of the ancestry of American Negro population, the geneticity of I.Q. and the relation between I.Q. and personality traits."

A main objection to this proposal arose on the ground that Shockley is an "electronics engineer" and not a geneticist. It also was argued that Shockley had published almost nothing on genetics in "respected scientific journals." Nevertheless, a majority of three ruled that "a Stanford professor who has worked vigorously in a field for half a dozen or more years—even without formal training in it—must be adjudged competent if he declares himself to be so."

Dean Moses, however, declared himself "unable to join the majority in believing that a Stanford professor .. . must be adjudged competent if he declares himself to be so." That is, he did not accept the thesis that a mature scientist must *necessarily* be a self-determining scientist. Mr Moses did not seem to realize that in the absence of self-determination, the adjudication of the competence of mature and recognized scientists must be assigned to boards of censors and thought controllers.

By overriding the majority of a committee convened by himself, Moses accepted the arguments of the minority. The minority simply found Professor Shockley as lacking in competence. Colin Pittendrigh, professor of biology and a former graduate dean at Princeton University, added that Shockley had not "responded adequately" to the "stringent" criticism which other scientists had leveled at his theories.[1]

The minority report alleged that Shockley treats his theories "as if they were solid enough . . . for purposes of making public policy and he has proposed what seem to be genocidal policies." Thus, the minority clearly made full use of its academic freedom to be willfully in error. It is an open question whether slander, like accusations suggesting espousal of "genocidal policies," is permissible under the rules of academic freedom.

The majority took a dim view of Shockley's alleged theory that the genetic potential for intelligence among Blacks is generally inferior to that of Whites.

Professor Maurice Krahl, chairman of the Stanford Department of Physiology, asserted: "The proposed subject matter of the course has the potential for strong emotional reactions in both students and the general public, so that the course most likely will be conducted in a highly political atmosphere."

On his part, Professor Pittendrigh argued:—

> One is led to understand that the scope of the course extends to cover the grand questions of the "future of humanity"

[1] Note that Shockley has for years been asking the U.S. Academy of Sciences to establish a research program to resolve the "environment-heredity uncertainty." Pittendrigh lacked familiarity with the case.

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and our " moral obligations " to study the rôle played by the evolution of the human gene pool in that future. The issues here are so immense, so diverse and so debatable that they cannot be treated at the graduate level in one quarter by any single scholar, no matter how well prepared he may be. The list of readings appended to the proposal consists almost entirely of pieces written by Professor Shockley himself, only two of which appear to have been published and none in refered journals of the first rank. The only items not by himself are the now famous paper of A. E. Jensen and one by R. T. Osborne entitled *Population Pollution*. Both men share Professor Shockley's strongly contested views on the extent to which racial difference in I.Q. performance tests are genetically determined. Professor Shockley's own statement of the course leaves no doubt that whatever its stated focus or thesis will be, the central theme will be his assertion that the " black minority " suffers from an innate disability with respect to intelligence.

Pittendrigh thought Shockley did not sufficiently demonstrate his competence to give " formal instruction in the many areas he intends to cover other than statistics." Before arriving at this conclusion, Pittendrigh " consulted with the appropriate department chairmen, with faculty colleagues in the areas involved including social sciences, and with other scientists of international repute in both human genetics and the analysis of human intelligence." He added: " The most relevant departmental chairmen are all (geneticists, psychologists, biologists) unwilling to include the proposed offering among those courses which they offer as their curriculum leading to an advanced degree."

Thus, the temptation to impose thought control proved to be widespread. Pittendrigh remarked that in reaching his opinion he was influenced by the knowledge that " no geneticist of stature—indeed none that I know of—regards Professor Shockley as adequately familiar with the complexities of the genetic problems to which he addresses himself." He did not indicate how, since Shockley supposedly did not publish, these gentlemen of " stature " determined the extent of the accused professor's knowledge.

It did not occur to the learned and conscientious Dr Pittendrigh that it is precisely a " phalanx " or " stone wall " situation of this kind which requires academic freedom, regardless of whether in the end the rebellious scholar will be proved to have been right or wrong. Who is able to predict the outcome of a debate about a topic which is as yet insufficiently researched?

The majority members of the committee basically shared the minority's opinion. Yet this critical view did not prevent the

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majority from approving Shockley's course. With strong concern for the realization of academic freedom at Stanford University, the majority said that the students would have an opportunity to subject Shockley's views to "open and objective examination." The minority preferred to believe that the graduate students would be "significantly poisoned by error."

The majority opinion reads: "As individuals we are fully aware of the controversial character of Professor Shockley's politication [sic] of what he calls the 'Dysgenic Question.' None of us is willing to accept his political views or his social philosophy." The majority adhered to the philosophy formulated by Voltaire: "I disapprove of what you say, but I will defend to the death your right to say it."

In connection with still another denial to teach, Stanford President Richard W. Lyman on 14th August 1972, suggested that the Committee on Undergraduate Studies "conduct an inquiry into the decision . . . to reject Professor William Shockley's proposal" to conduct a "workshop" entitled *I.Q., Geneticity, Heritability, and Correlation with the Quality of Life*. President Lyman observed that "the reasons for the decision . . . are deeply troubling." He rejected the notions that the sensitivity of the problem and the controversial nature of the subject are adequate reasons for turning down the proposal. He called attention to the fact that the type of "workshop" Shockley was offering was customarily "led by a diverse group of individuals, from undergraduates to professors and professional people." Accordingly, President Lyman doubted whether "the standard of competence by which Professor Shockley was judged is the same one used to assess the competence of other . . . instructors."

The American Civil Liberties Union of Northern California decided to intervene and on 19th September 1972 addressed a letter to university authorities. This letter reads in part:

> Four reasons have been given Professor Shockley for the rejection of his proposed workshop:
>
> 1. Morality/sensitivity—the first questions raised were "the issue of morality and moral decision-making in an action-oriented program" and the "sensitivity of the issues involved," which led the Board to question whether the "provocation of discussions and a search for social remedies would contribute to the solution of a social problem."
>
> 2. Professor Shockley's competence to lead the workshop.
>
> 3. Possibility of disruption of the workshop.
>
> 4. Suitability of an undergraduate course which is based largely on highly sophisticated statistical methods.

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Reasons 2 and 4 do not present civil liberties or academic freedom *per se*. The decision to reject a workshop on the basis of an instructor's competence or course suitability may be acceptable provided that it is made in good faith and not used as a subterfuge for the denial of civil liberties.

MORALITY/SENSITIVITY

First, to deny sanction for a workshop because of the morality or sensitivity of the issues involved strikes at the heart of the principles of academic freedom which are the foundation of a great university. Freedom of inquiry into controversial issues is essential in a democratic society. Only through full and free inquiry can the validity of controversial theories be tested.

Second, Stanford has consistently and properly denied the propriety of casting moral judgments upon research or course offerings . . . Were Stanford to adopt " morality " as a criterion, a more precise definition of the standards to be followed must be made. The letter of rejection contains no such definition. Neither is " morality " included within the Policy Board Criteria, dated 2/29/72. Certainly " morality " as a criterion presents a danger in that personal notions of morality and taste may be used arbitrarily to reject controversial course offerings.

Third, the fact that the proposed workshop deals with sensitive and controversial issues *should be a positive factor in the decision process*.[2] Traditionally the University has been the very place such issues have been raised and studied. The University has an obligation to encourage the expression of heterodox, " dangerous " thoughts. To deny sanction for a workshop on this basis would violate the tradition of academic freedom. In fact, the Policy Board Criteria (2/29/72) explicitly recognise this factor. Section B.4 states: " Does the workshop deal with a social or political issue, i.e., a matter of current concern to more than the Stanford community and one that normally involves a controversy?" Certainly Professor Shockley's proposed workshop meets those criteria. To *deny* approval on the same basis is nothing short of hypocrisy.

POSSIBILITY OF DISRUPTION

No consideration should be given to the possibility of disruption of the workshop. Where third parties constitute a threat to disrupt a class or workshop it is the responsibility of the University to take whatever steps are necessary to insure the right of faculty to teach their courses. Disruptors should be

[2] Italics added.

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dealt with by appropriate disciplinary bodies. An analogy may be drawn with speakers protected by the First Amendment. There, the government has an obligation to protect the speaker and may not justify suppression of speech on the basis that there is a possibility of disruption where there is no incitement to unlawful action.

On 6th July 1973 the Committee on Undergraduate Studies vindicated the original decision that had been made by the Policy Board, Stanford Workshops on Political and Social Issues (SWOPSI). The question by President Lyman was answered to the effect that the criteria which were used to judge course proposals, " were applied in a consistent fashion " and " that the criteria used in judging Professor Shockley's course were consistent with the requirements of academic freedom."

Several interesting facts came to light in the report of the Subcommittee which had wrestled with the problem.

1. The SWOPSI Policy Board which in 1972 rejected Shockley's course was composed of five faculty members, five students and one community person. The vote was four in favor of the course, three opposed, and two abstentions. Thus nine of eleven members voted. It is known that one of the members moved away from Stanford. No information was provided on why the eleventh member failed to vote.

2. Shockley obtained 57 per cent of the effective vote. But incredibly, the board required votes by two-thirds majority. The course was rejected on the strength of this voting rule, that is Shockley was blocked by a minority.

3. Within the board four members were supposedly competent to judge the course proposal. One was a recognized expert in developmental psychology, and one an expert in mathematics and statistics. Two others were described as having advanced degrees in psychology which probably means that they were graduate students. No member was a specialist in genetics. One of the four expert members resigned before the vote in order to teach elsewhere. Two experts abstained, and only one expert voted against the proposal.

4. Since nine votes were cast altogether, it follows that four non-experts voted for Shockley, one expert and two non-experts against, and two experts abstained. The subcommittee reported that the " four members who voted in favor . . . did so principally because their concern for academic freedom outweighed such negative factors as may have been relevant." It seems likely that students were included in the affirmatively voting group.

5. Of those who voted against, two voted in this fashion because they doubted Shockley's expertise, and one because he

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doubted expertise also, but thought additionally that the workshop " would have undesirable social consequences."

6. One of the abstainers could not decide how to weigh academic freedom against lack of expertise and the likelihood that the workshop would not be an educational success. The other abstainer thought the board should not have voted before consensus was reached.

7. Those questioning Shockley's expertise were influenced by " peer judgment " of outside experts. The one voter who was swayed by the possibility of untoward " social consequences " was impressed by the fact that the majority of Dean Moses' committee had viewed the policies Shockley " seemed to recommend as essentially genocidal." There was no check of the justification of this view. Yet it was precisely this view which determined the vote, together with the two-thirds rule.

8. It turned out that the letter by which Shockley was notified of his rejection, was irregular. Written by a student who had acted as chairman of the board, and forwarded unsigned, it was not approved by the board. The director of SWOPSI did not agree with the letter but signed it anyway, because a notification to Shockley was overdue. The letter, the committee noted, was " damagingly misleading."

9. The subcommittee which investigated the board's decision was composed of one political scientist, one aerophysicist, one pharmacologist-biologist, one graduate student, and one sophomore student in education. None apparently, knew anything about the subject matter of the proposed course. The names of these five persons appear in a typed form at the end of the report which carries no signatures. The graduate student (whose connection with the university is unclear) " agrees with the findings and conclusion of the report, except that she neither concurs nor dissents from the conclusion concerning Professor Shockley's competence." The astrophysicist appended a supplemental statement explaining that " the Policy Board's deliberations were at best only partially consistent," and that they were actually " inconsistent in that different and basically higher standards of competence were being required [of Shockley] as compared to those of instructors in other SWOPSI courses."

10. His alleged lack of competence was the key issue on which Shockley lost. This was simply a pretext: the allegation is patent nonsense and the application of *this* argument to his proposal was inconsistent with SWOPSI practice. The persons who sat in judgment on Shockley's competence lacked competence themselves, and they made no attempt to judge the case on its merits through the application of scientific methodology. Instead they relied on

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opinion, much of which was imputed to outsiders, as well as on rumors, misquotes, and misunderstandings; and they also leaned on slanderous allegations by another board without subjecting those to critical judgment.

Despite those unfavorable circumstances Shockley obtained a majority. He was, in fact rejected by an anonymous voting procedure, the wisdom, necessity, or origin of which was not explained.

This academic inquisition lacked a Cardinal Bellarmine. As fitting modern times it took the form of a bureaucratic snafu or blockage, with no one identifiable responsible.

The story is hardly finished. After the report of 6th July 1973, the situation may be summarized as follows:

1. Academic freedom was violated by one of America's leading universities.

2. This violation did not lead to actions spontaneously generated by the faculty to correct the censorship.

3. When the matter was submitted to a formally constituted group for reconsideration, the violation was "explained" away and rationalized to the point where the violation was denied. Thus, the violation was confirmed and continued.

Are the thought controllers fascists or communists? Are they, on whatever principle, opposed to academic freedom? Neither. They merely disagree with Shockley for political and emotional, and *perhaps* also for a few scientific reasons. They are entirely free to disagree. But they are not entitled to silence the person with whom they disagree. They most certainly are not justified to be so arrogant or so hypocritical as to deny the competence of a world-renowned scholar, especially when they did not bother to familiarize themselves with his work.

The will to muzzle a dissenting voice usually results from sub-conscious or conscious awareness that the dissenter may have a point or two which the guardians of orthodoxy find impossible to disprove. Otherwise the *Gleichschalters* would extol his unrestricted freedom to speak, teach, and write. And they would confidently await the inevitable moment when the dissenter himself reveals his incompetence and his voice is drowned in a gale of laughter.

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Good, Lyman Express Views on Shockley Course Decision

The following letter from Stanford University President Richard W. Lyman to Professor Edwin M. Good, chairman, Committee on Undergraduate Studies, appeared in Stanford's *Campus Report* on 8th August 1973.

" I've been at something of a loss to know how to comment on the report of Professor Marshall's Subcommittee which you were kind enough to forward to me on behalf of CUS [Committee on Undergraduate Studies] on July 6, 1973. The report is long and involved; clearly it represents the result of lengthy deliberations and considerable effort of inquiry, for which I am grateful. Clearly also there is a point of diminishing returns in the further pursuit of the subject of Professor Shockley's application to SWOPSI [Stanford Workshops on Political and Social Issues], its rejection by the SWOPSI Policy Board, and the explanation given on behalf of that board by Dan Lewis in his letter of July 10, 1972.

" By the same token, however, my main purpose was not so much to retry the issues of Professor Shockley's particular case — as I have said from the outset, I am in no position to judge on its merits the decision to reject his application—but rather to raise important general questions about the policies and operating principles of the SWOPSI Program, questions that seemed to me to be raised in rather acute form by Mr Lewis's letter. I cannot hope to deal with all that the Marshall Subcommittee has to say on these questions, but I would like to make a few comments on two major issues considered by the subcommittee, the question of assessing competence and the question of academic freedom.

" In any university, the evaluation of the potential and performance of members of the faculty involves the most difficult and most important set of decisions made. Difficult as those decisions are when they are within the framework of a given discipline, they are even harder when what is to be evaluated is the competence of an individual to teach in an area outside his regular one; and when those doing the evaluation are not themselves experts in the field of the proposed course. I gather that this situation is not uncommon in SWOPSI's experience; indeed, to the extent that SWOPSI encourages substantive innovation, this situation is at the heart of the SWOPSI program.

" In the more typical SWOPSI proposal, however, the proposed instructor is unlikely to have a substantial record of performance in any field, and the board must then rely on the " sponsorship "

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of a faulty member whose field of specialization may be more or less near the subject of the course. Professor Shockley's case was exactly the opposite: he has a distinguished record in his own field, and an extensive but controversial one in the subject he proposed to teach. My reading of the committee report leads me to conclude that the Policy Board never came genuinely to grips with the anomaly of their situation: on the one hand they are willing ordinarily to take great risks in certifying wholly unknown quantities, on the other they were willing to risk nothing on an individual whose record was extensive, but about whose record there was controversy. One wonders, indeed, whether the same course proposed by someone unknown to the SWOPSI Board might not have had an easier time.

" Needless to say, I am not urging lower standards of judgment for some because the absence of information makes judging less rigorous in the case of others. Professor Bershader focuses on this discrepancy in standards of judgment in his supplemental statement. On the record as surveyed by the subcommittee, I am inclined to agree with his conclusion that the board was ' at best only partially consistent,' and that its inconsistency lay ' in that different and basically higher standards of competence were being required (of Professor Shockley) as compared to those of instructors in other SWOPSI courses.'

" The significance of this point extends beyond the immediate Shockley case. The proper concern of the faculty is with the intellectual quality of the curriculum, and central to that concern is the quality of instruction. Criteria for judgment may change over time, and there may even be a case that SWOPSI's purposes require that various criteria be given more or less weight than they would be given in other situations. What cannot be defended is a situation in which the standards and methods of judgment are so loose as to shake one's confidence that they can be applied with even handed rigor. I am disturbed by the subcommittee's seemingly rather speedy dismissal of the question of consistency in the SWOPSI Policy Board's considerations of competence. Reference to the existence on file with CUS of ' a supporting memorandum verifying the fact that the SWOPSI Policy Board routinely considers the expertise of the instructor/sponsor ' strikes me as hardly a sufficient answer to this crucial question.

" The committee report is, in my judgment, least satisfactory in its discussion of the implications for academic freedom in the Shockley case. The report considers at length the question ' whether a regular member of the faculty has a " right " to teach a course *for credit* outside his department and outside the professional field in which he is credentialed.' Not surprisingly, the committee finds that no such ' right ' exists. I would go further

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and say that no such right has been asserted. Certainly my memorandum of last August contains no such assertion, for the reason that I believe the proposition to be absurd on its face.

" The issue of academic freedom which *is* raised in this case is whether a member of the faculty was judged by the same standards as those used to judge others in the program or whether different standards were applied because the substance of his views were distasteful to those doing the judging. The record does not support an unequivocal answer to that question, but the fact that at least one board member was moved to a negative vote in part by concern for the Workshop's ' undesirable social consequences ' is evidence that the question is not a frivolous one.

" Further, the subcommittee report appears to me to raise a fresh and again disturbing question about the application of criteria by the SWOPSI Board. On p. 14 of the report, a point is made of the need for ' a substantial degree of cooperation among members of the [Workshop] group. There must be agreement on the propositions or hypotheses to be tested, the data to be collected, *and ultimately on the conclusions to be drawn from the data.*' [italics added]. Apparently, the anticipated lack of such agreement, plus the fact ' that a substantial number of students planned to enroll with the intent, at best, to challenge Professor Shockley or, at worst, to confront him,' caused at least one member of the Policy Board to vote against accreditation. I find here at least the seeds of a notion so antithetical to academic freedom as to cause surprise that the subcommittee apparently found nothing worrisome about such statements. I should have thought that ' challenge,' and even ' confrontation,' as long as it implies no interference with the functioning of the University, were to be prized, not avoided. The possibility that for some, at least, SWOPSI is to be an exercise limited by the requirement of concensus among Workshop members is ominous.

" As I trust I made clear at the start of this long letter (for the length of which I do not apologize), I do not intend to imply any desire on my part that either CUS or the subcommittee pursue the particular historical circumstances of this case any further.

" The wide interest generated by this case is testimony to the importance of the SWOPSI experiment as well as to the significance of the issues in the particular case. The program deserves, and I trust will receive, the most careful consideration of which we are capable. The record of this case should prove useful to future SWOPSI Boards and to the faculty as a whole."

PROFESSOR GOOD'S STATEMENT

The following letter by Professor M. Good, Professor of Religion, of which excerpts are made, appeared in answer to

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President Lyman, in which he alleged the incompetence of Professor Shockley to teach this subject.

" . . . the President believes that the Marshall subgroup gave a ' seemingly rather speedy dismissal of the question of consistency.' I am at a loss to know what leads the President to this conclusion. In fact, it is not true. Mr Bershader's additional statement, addressed precisely to this issue, indicates that the question continued to occupy the subgroup for a long time, and CUS itself, in two lengthy meetings on the subject, spent quite a long time over it. Indeed, the very memorandum that the President dismisses as ' hardly a sufficient answer ' to the question was drawn up by Professor Marshall at the request of CUS. That memorandum is and will remain confidential, and therefore it was not made a part of the report. The reference to it as a footnote to the report was intended as a message to the President that he ought to ask for it, and that message was reinforced by at least two further oral messages about that and another memorandum to the President through his assistant. The President did not ask to see the memorandum however, and he can hardly dismiss it as an insufficient answer. As I say, that memorandum will remain confidential but it gives evidence, with names, dates, and titles, of proposals that the SWOPSI Policy Board rejected because of lack of confidence that faculty instructors or sponsors were competent to handle the workshops they proposed. CUS intended that the President have access to the memorandum, but he chose not to take it. Had Professor Marshall and I been invited to discuss the matter, we could have underscored to him the significance of the material, and perhaps his anxiety would have been alleviated.

" I fail, moreover, to see that the question, whether a faculty member has the right to reach for credit outside the area of his credentialed expertise is an absurd question. An answer to that question seems to me logically necessary to establish that the Policy Board had the right, on application of its chosen criteria, to reject the proposal of a faculty member. Perhaps the answer to the question is, in this day and age, more or less obvious, but that does not make the question absurd. I have not checked back to the documentation, but I have the impression that the *ad hoc* committee that reported to the Graduate Dean about Shockley's proposal to teach a Graduate Special Course considered precisely that question and did not treat it as absurd. In my view, the question is not absurd precisely because innovation is an important element of education, and a constant re-examination of the question of the right to teach outside one's field seems to me to be the only way in which we can assure ourselves that innovation has genuine quality.

" The President is still worried that the Shockley proposal was

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rejected ' because the substance of his views were (*sic*) distasteful to those doing the judging.' These views were indeed distasteful, and nobody hid the fact.

" The President can, if he wishes, guess that that problem lurked in the minds of Policy Board members, but it is at best a dubious compliment to the subgroup to say to them, in effect, " I don't really believe that the Policy Board members really told you their real thoughts on the issue." To conclude that, it would be better never to have had an enquiry. The subgroup found (and no member of the subgroup hinted at dissent on this) that distaste for Shockley's position did not determine anybody's vote. I believe that, and I think the other members of CUS believe it. If the President does not, that seems to put the burden of proof on him —a burden he explicitly wishes not to have. His statement, then, is at best incautious.

" As for the fact that one member was moved—partly but not entirely—by the question of undesirable social consequences, I believe that the President has not read the report with enough care. The nature of those consequences is very carefully spelled out in the report, and once again reference is made to a memorandum, also confidential, that details other instances in which proposals were rejected on those bases. The term, " undesirable social consequences," of course, covers a lot of ground, but as used by one member of the Policy Board it is not quite so broad as it may seem. Once again, the President did not make any effort to see the memo that gives the evidence about the application of that criterion, and it is hard to see, even on the basis of what the report says, how he could draw the conclusion he does from the term. I disagree with his conclusion and so, I believe, does CUS, unanimously.

" . . . the President finds it ominous that the SWOPSI Policy Board is really interested in good education. The phrase that he underlines could certainly have been written so as to avoid the misinterpretation he put on it, and the misunderstanding once more suggests to me how valuable would have been his inviting us to discuss the matter. Had I projected a misunderstanding of that phrase, I would have suggested revising it, but I did not. Had the President told me his understanding of it, I could have assured him that that was not what the phrase intended. The phrase is ' and ultimately on the conclusions to be drawn from that data.' The critical word there, as I read it now and did when it was before the committee, is ' ultimately,' and it refers to such time in the process of a Workshop when enough data are in that conclusions can be drawn. Certainly, neither SWOPSI, the Marshall subgroup, nor CUS is interested in offerings that reach conclusions before the research is done. The point of the statement, of which this is

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part, is a much more important issue of educational philosophy than the President seems to see. And that is that learning proceeds under assumptions of good faith and intellectual seriousness on all sides, or else it is not learning.

" The Policy Board, as is quite clear from the report, was not worried about confrontation or lack of unanimity. I was worried about good faith and intellectual seriousness, and it had what seemed good evidence that a large proportion of the enrollment in the course was going to be by persons who were there not to learn in good faith but simply to confront. Moreover, as I read the report, I understand that the Policy Board was less than convinced that Professor Shockley had the pedagogical capacity to turn that sort of confrontation in the direction of genuine learning. It comes back down, then, to a lack of confidence on the part of some members of the Policy Board in Professor Shockley's pedagogical competence—now not his competence as a thinker and researcher in genetics but his competence as a teacher. It seems to me quite reasonable to raise such questions if one may fairly predict that that particular pedagogical situation is going to arise. And if it will likely not be well handled, then the Policy Board would be in the position of offering a workshop that did not involve genuine learning. I do not think that avoiding such an eventuality proposes something ominous at all, but rather that it is a credit to a Policy Board that must be concerned with the academic quality of its offerings. Certainly challenge is to be prized, but not for its own sake. It is to be prized and sought out when the necessary conditions of intellectual seriousness and good faith are present, when, that is, the challenge is to the intellectual material and not to the person involved. That was the issue the Policy Board had to face, and I believe they faced it."

* * *

In fact Professor Shockley is a particularly good teacher, who has published books on teaching methods in the natural sciences.

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Subversion of Academic Freedom

By STEFAN T. POSSONY

Freedom of speech is a basic human and political right. Without this freedom the individual cannot be free; and society cannot be free either. In the absence of critical analysis, conditions cannot be criticized effectively, political opposition to policies, governments, states, and social systems cannot be formed, democracy cannot function, conditions will not be improved, and progress in all fields of human endeavor will be retarded. Freedom of speech is the key to freedom and its success.

By contrast, denial of free speech is the *conditio sine qua non* of despotism. Such denial is required before a despotic system can be established, and continuous denial linked to propaganda and brain-washing is needed to preserve the despotic structure.

Thus, freeom of speech is more than an individual right, it is a social necessity.

No right, however important, is absolute. Despite its tremendous importance freedom of speech must, therefore, be restricted by the rule that it must not trespass on other rights and on the rights of others. The practitioner of free speech is morally bound to stick to the truth and to be honest. He is also obliged to observe limits of a practical nature such as tact, propriety, public safety, respect for privacy, etc. Obscenity, mendacity, slander, blackmail, panic-mongering and incitement to crime are not regarded as legitimate exercises of the right to free speech.

Academic freedom, or the freedom to study, teach and publish, is an element of freedom of speech. It is needed, *inter alia,* to overcome entrenched error and to ensure the advance of knowledge and understanding.

Freedom of speech belongs to all citizens, including illiterates and even morons. Academic freedom applies only to persons who are using objective arguments and rules of evidence. The particular right to teach is based upon acquired and demonstrated competence.

Academic freedom, too, is not absolute. Universities admit as teachers only persons who earned the credentials of expert knowledge in a specific field. The examination system, through which the basic credentials are obtained, is based on the explicit assumption that the qualified teacher normally knows more than

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his students and that it is he who decides what is right and what is wrong. The student who cannot provide the right answers "flunks" the test and does not acquire the qualification to teach.

Furthermore, universities prohibit courses which instruct in, and incite to harmful, immoral, and criminal actions. They reject charlatans and quacks, and do not, for example, teach that cancer is best cured by drinking green tea or that economic or political decisions are best based on astrology. The task of a university is to transmit and create verifiable knowledge.

Such knowledge is not restricted to the type of "positive knowledge" which characterizes mathematics and the natural sciences, but has historically included the social sciences, philosophy, metaphysics, and theology. The main rule is that scientific methodology must be used and that on controversial questions different and divergent views are to be presented fairly.

The fact that this rule is not always observed does not mean that the validity of the rule is under dispute. Indeed, the commitment to the propagation and production of truth, *i.e.* verifiable knowledge, poses many difficulties. The history of science shows that very few "truths" remain valid forever and that sooner or later most are superseded by "better" or more complex truths. The new truth, especially if it is of a basic kind, usually must fight for acceptance. Even pedestrian new findings and solutions often were the object of prolonged controversy. Customarily there is a majority which speaks "authoritatively" on the subject, while one or more minorities propose alternate interpretations. Since the ultimate scientific resolution of competently conducted debates is unpredictable, dissenting views must never be stifled and no searcher must be prevented from pursuing his line of reasoning. All assertions and hypotheses must be tested by logical and factual evidence, and none must be dismissed by non-scientific methods, such as pre-judgment and muzzling.

In practice, the spokesmen of "heretical" views frequently are given a hard time. Their competence is questioned and their careers are impeded. Their writings are unmercifully and sometimes wrongly and even maliciously castigated; not infrequently a critique of the criticism is prevented. Academic politics can be as intolerant and irrational as power politics. They are getting particularly nasty whenever assumed or axiomatic "first principles" and premises are involved, from which the success of so-called "schools" was derived or which have a bearing on political, social, and religious questions.

Intellectual oppression is a hallmark of totalitarianism, but it equally occurs, albeit furtively and deceptively, in democratic states where unpopular views also tend to invite suppression and sanctions. American and British professors claim undying

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dedication to academic freedom and regard the defense of this freedom as a primary obligation. But all too often that freedom was claimed only for favorite "liberal" causes. The left dissenter was assumed to have something worthwhile to say; the right dissenter was deemed to be incompetent and hence not worth listening to. The gratuitous allegation of incompetence or of other pejorative characteristics does not, of course, provide any license for muzzling. Unfortunately, though the substitution of *ad hominem* affirmation for reasoned argument should be taboo, academic careers often depend on subjective evaluations and extraneous judgments.

Editorial boards and academic administrators tend to exercise coercive conformism as a matter of habit and "efficiency." Much unwelcome work can be kept away from print on technical grounds such as "no space" or "no public interest." But there may be explicit blacklisting of "discredited opinions" and nonconformist authors by academic potentates who claim they represent the "final judgment" or the "decent" view.

In addition to blacklisting, reputations may be blackened through the instrument of distortions and deliberate misunderstandings in book reviews. In such academic moot trials against the heretic, prosecutors and judges are often the same person; the accused themselves are seldom heard. No rules of evidence are adhered to, to guard against misrepresentation or to determine the validity of arguments and counter-arguments. Yet the hostile review in an "authoritative" organ may harm and silence the heretic.

The procedures of the academic inquisition are strictly informal, behind closed doors, and they are predominantly based on innuendo, rumor, hearsay, and misquote. Few inquisitors ever know, or want to know, what the defendant really said or meant, or would understand his points if they were familiar with them.

A classic case is that of Immanuel Velikovsky who criticized evolutionist theories in astronomy, astrophysics, and geophysics. Those theories were regarded as fully established and beyond argument—in the believers' minds, they had become indisputable facts. Since the work of several generations of scientists[1] was involved, Velikovsky's logic, hypotheses, and findings were dismissed out of hand. His publisher was forced to stop issuing his books. Velikovsky was denied academic platforms, and for years he was subjected to slander as being a charlatan or worse. Yet space exploration confirmed many of Velikovsky's anticipations; he was the only man to make those particular

[1] The credibility of ancient texts, including Babylonian astronomy and the Bible, as well as the dating of ancient, especially Egyptian history, also is involved.

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predictions.[2] The surprised experts got busy inventing *ad hoc* theories to explain away the disaster. The ostracism which had lasted for more than 20 years was broken in 1972 when Velikovsky was invited to lecture at Ames Research Center, U.S. Space Agency. The magazine *Penseé* was founded to explore his theories and he was invited to lecture in academic institutions. The ice was broken, but there still was plenty of ice.

Velikovsky's books which display self-evident erudition and scientific probity, sold well. This enraged his opponents who, incredibly, sought to have his books withdrawn. Yet many of those opponents never read a single line or chapter written by Velikovsky and therefore their criticism was necessarily incompetent. It usually is true that persons who lack formal training in a particular discipline lack competence in that field. But persons who criticize and oppose that which they have not read, studied, and evaluated through proper methodology, clearly lack competence as scientists regardless of their credentials and irrespective of the disciplines and issues that may be under debate. Scientists can be regarded as *bona fide* scientists only *if, when,* and *while* they are sticking to the rules of science and they cease automatically to be scientists *whenever* they ignore those rules.

The question is not whether Velikovsky was right, half-right or wrong—I cannot judge most of the issues involved—but whether in his case academic freedom was infringed on a massive scale and in an inquisitional manner. Velikovsky had plenty of credentials, and "authorities" like Einstein felt, nearly from the start of the controversy, that he should be given a hearing. Einstein also recorded on paper that "the roaring astronomical lion" who led the campaign against Velikovsky, was "not showing enough respect for the truth."[3] To make things worse, during the period of ostracism, borrowings were made from Velikovsky's writings, always without attribution. The Space Agency found it worthwhile to maintain informal contact through Hess who transmitted Velikovsky's suggestions. But there was no "roaring lion" who spoke up for Velikovsky's rights.

The reasons for the attacks on Velikovsky were emotional: established creeds were challenged by a deviant thinker; and the

[2] The late H. H. Hess, chairman, Geology Department, Princeton University, and chairman Space Science Board, National Academy of Science, wrote in 1963: "Some . . . predictions were said to be impossible when you made them. All of them were predicted long before proof that they were correct came to hand. Conversely I do not know of any specific prediction you made that has since been proven to be false." These predictions number in the dozens. See Thomas Ferté, "A Record of Success," *Pensée,* Lewis and Clark College, Portland, Oregon, May 1972, p. 11ff.

[3] Einstein letter of 17th March 1955 to Mr and Mrs Velikovsky. See *Pensée,* May 1972, p. 39, where a facsimile of the letter appears.

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spokesmen of orthodoxy *knew* they had no answers to many of the questions the heretic was raising. The incident disclosed, not only a deficient scientific ethos, but also incredible lack of intellectual stability and self-confidence. *Auto-da-fés* are used to destroy books that instill fear.

Other types of infringement occur because of politics and scientific fads. The anthropologist Carleton Coon was subjected to political attacks because of his hypothesis that the Negroes may have been the last race to evolve into *Homo sapiens*. Rather than fight the battle, he chose to avoid further damage to his health and withheld from publication an extensive analysis of racial differences in brain anatomy.

Other examples. The story of I.Q. testing is marred by fluctuations of praise and rejection. Psychoanalysis was first rebuffed by the American academe, later it was allowed to dominate, to the detriment of other doctrines in psychology, sociology, and cultural anthropology. In economics, the story of the (usually misunderstood) teachings of J. M. Keynes is similar: his school became dominant, the classical schools were downgraded, and the school of Ludwig von Mises was ostracized. There are many additional examples, such as the disputes between "orthodox" historians and "revisionists" and their respective evaluations of the origins of the two world wars and the cold war.

The customary, though usually inaccurate reference to Galileo is convincing only when it is not used exclusively to hit the Church of the past but when it serves to expose the inquisitions practiced by self-styled "objective" scholars of the present time.

* * *

William Shockley is one of many contemporary Galileos. The penalties to which he is exposed are less gruesome than those which menaced Galileo and which killed Giordano Bruno, and to that extent we may rejoice about progress. But the *modus disputandi* has not improved, on the contrary. The spirit which aims to decide scientific controversy by force and power survives, undiminished in energy and strengthened by devious tactics. Oppression is preached and executed in the name of science.

Shockley is concerned about the social impact of a high and possibly growing frequency of insufficient human intelligence (below 70 I.Q.). On the basis of impressive evidence presented in the "authoritative" literature, he regards intelligence as mainly derived from genetic endowment. Hence, logical extrapolation leads him to believe that welfare programs, which cost ever more billions of dollars, are useless in some of their undertakings and may aggravate the difficulty. The programs about which he is skeptical are based on the assumption that low intelligence is

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primarily due to environmental factors, such as deprivation and racial hostility (" racism ").

It is noteworthy that none of his academically qualified critics has risen to state unequivocally that intelligence has *no* genetic base and is entirely caused or shaped by environment. One critic wrote: " When Dr Shockley says heredity is more than twice as important as environment . . . he doesn't know what he is talking about . . . Both variables are *completely* important. Any other statement is nonsense." Shockley said that for 244 separately reared, Caucasian, identical twins, compiled from four independent studies, the best estimate of geneticity (which means parental heritability plus non-hereditary gene variance) is 82 per cent. Dr James Crow, like Shockley a member of the U.S. National Academy, wrote that geneticity " values from 0.60 to 0.85 could be defended " and that heritability estimates of 45 to 50 per cent as set forth by Christopher Jenks of Harvard, are too low.[4]

Thus, there seems to be a clear need for the resolution of a problem that has been discussed time and again; and which, as the Jenks book proves, is regarded as important by many people, liberals included. The argument that the two variables of geneticity and environment are both " *completely* important " seems to reflect semantic gamesmanship. For example, both the brain and the heart are " completely important " for human life; but each organ can and must be handled separately before their interplay is understood, and naturally if one of the two organs is diseased it must be treated by methods that are specific to the particular organ. Similarly, a report endorsed by the Academy stated that genetic factors are " highly important " with respect to some problems, " while with respect to others, they are unimportant." This truism, however, did not lead to a description of the areas of supposed importance and unimportance. In any event, it should

[4] Christopher Jenks in *Inequality,* New York, Basic Books, 1972, Appendix A, provided several estimates and ended up with .45 $\pm$ .10. He added: " The margin of uncertainty remains considerable." In connection with twin studies, he thought that those " point to heritabilities in the .45 to .60 range, with a value of about .55 seeming most likely." He also thought that " the chances are about 19 out of 20 that heritability is between .25 and .65." He produced a survey of 14 British, American, and Danish studies of identical twins reared apart which indicate a range of uncertainty of 37 points between .49 and .86. Jenks, incidentally, believes that it is a desirable objective to design " ingenious machinery for taking money from the rich and giving it to the poor." Thus, an objective equalitarian socialist who is respectful of science did not hesitate to accept heritability as a fact; and his work disproves the contention that the interrelationships between heredity and environment cannot be studied effectively and, therefore, should not be studied at all. Incidentally, in a different language the problem was discussed by ancient philosophers and symbolists. See René Guénon, *La Grande Triade,* Paris, Gallimard, 1946, second edition 1957, especially chapter XIII.

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not be used to obfuscate the specific problem posed by Shockley and others, *viz.*, to what extent is mental retardation due to geneticity?

On the other side of this debate, there is no agreement on the specific meaning of "environment" and on the mechanisms through which environmental factors form or affect intellectual capacity. No distinctions have been made between the several distinct types of environment that could be relevant, despite the fact that there are numerous different environmental theories. The term, "environment" is, at least in part, due to a *Weltanschaung* which surfaced during the eighteenth century in John Locke's concept of the *tabula rasa*. This does not signify that the environmental concept is necessarily wrong, nor that environment is not an important factor that requires treatment. But at this time no explicit arguments and scientific proofs exist to establish the validity of programs which aim to enhance intelligence by manipulating select environmental factors; and the wisdom of the selection of those factors, and of the ignoring of others, has not been established either.

Shockley does *not* claim he *is* right, he merely says that plenty of indicators suggest that he *may* be right. He also calculated that if geneticity were only 70 per cent, the probability that the samples are misleading, on which the 82 per cent estimate was based, would be only 1:2,000. Hence he has been proposing to the Academy that it review his calculations of significance levels and that the problem be properly researched. Thus, he is not "controversial" because he proclaims a "new truth" but because he stresses the importance of a problem, which others see just as well but prefer to ignore. The controversy is about a research proposal and about Shockley's arguments in favor of the proposal. After seven years of evasiveness and intransigence the Academy's stand has been weakening, to some extent. But, so far, it still refuses to investigate the causation of low intelligence.

There cannot be any scientific reasons for this astounding refusal. The problem is of the highest scientific interest; and a conclusive finding either that geneticity is a decisive cause of stupidity or that retardation is predominantly environmentally induced would entail considerable practical consequences.

The reasons for the Academy's refusal are, in the main, political. Lest there be doubt on the matter it should be recalled that geneticists and environmentalists have been battling for several generations, and that both positions were set forth by highly skilled spokesmen. The nearly unbroken continuation of this controversy suggests that an important issue is at stake. At the same time, continuing lack of resolution suggests that knowledge is incomplete and/or that methodology is deficient. What can be more reasonable

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than to refer the matter to the Academy of Sciences? What other institution would have a better chance to solve the problem and to preserve full objectivity?

Assuredly, since the subject bears on the status and future of American Negroes, Indians, and Chicanos, etc., and numerous minorities all over the world, it is explosive; a finding which confirms the key rôle of heredity may indeed result in misinterpretations and untoward agitations. But by the same token, a finding which disproves Shockley's worries would be welcomed by the minority groups who expect their salvation from environmentalism.

It is hard to believe that the Academy did not see the political attractiveness of the Shockley proposal in terms of demolishing, once and for all, the customary genetic justification of "racism." Accordingly, within the context of their own political perceptions and of contemporary public debates on race problems, the Academy presumably would have accepted Shockley's suggestion if, in point of fact, the majority of its members did assume a careful scientific investigation would confirm the environmental thesis. The repeated refusal to look into the matter inevitably leads to the presumption that the Academy's majority does *not* believe the environmental thesis can be validated. But perhaps such a presumption assumes too much logic. It is more likely that the majority acted largely on the basis of prejudgment.

The U.S. white majority includes a large number of "retarded" persons, and their handicaps will grow worse as the professional requirements of a technological society become increasingly severe. Hence the problem concerns *all* races and ethnic groups, and apprehensions about the sensitivity among minority groups, while justified in themselves should not be a decisive factor.

It is understandable that social and ethnic groups which are deemed to suffer from intellectual handicaps are touchy on the subject, especially if there are grounds to fear that such handicaps may be exploited for purposes of domination. Still, if there are good reasons to assume that the handicap may be real, this assumption should be verified or be rejected for cause. Measures to eliminate the trouble can be devised only if and when there is a proper diagnosis of the handicap's nature. The threat of oppression can be neutralized only if the handicap is overcome or its nonexistence is established.

A costly cure without knowledge of what it is that is to be cured characterizes the present approach. Instead of playing the ostrich, the Academy should have issued the requisite warnings and it should have provided constructive guidance. After all, in solving this problem in one way or the other, the Academy would render outstanding humanitarian service.

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The opposition to Shockley is specifically motivated by the wish not to antagonize American Negroes. However, so far the Negroes have shown little interest in the controversy. The ostracism has been mainly a "cause" of white activists disinterested in productive research and intent on fighting "fascists" and "racists."

Opposition is furthermore rooted in the fear that if heredity were dominant over environment, the social problems resulting from a high frequency of stupidity might be virtually insoluble. Yet it would appear that a dysfunction which can be treated by medical techniques—such as Alpine idiocy, which was eradicated, simply by adding iodine to water—is more easily manageable than a problem which can be handled only by limitless environmental manipulation and structural changes of social patterns and cultural traditions. For that matter, directed manipulations and changes may not be feasible and strong attempts to "change society" may lead to large scale unrest and violence.

Unfortunately, exaggerated confidence in the potency of environmental manipulation is a powerful myth of our era. Outside academe, the devotees of revolution are dogmatically dedicated to total environmentalism, and they rejoice at the partial expropriations of wealth which over-sized welfare programs entail. The revolutionary potential of scientifically ill-conceived social-environmental programs that are aimed at transforming society in order to change low into high I.Q. types is enormous.

The problem which Shockley wants the U.S. to address is genuine and important. The danger of politicization exists but it would be mitigated once it were realized that the proposed research is designed to help, not to hurt. In whatever form, the findings would be useful to all societies, political orders and social classes, and that would be so irrespective of whether conservatives, liberals, or revolutionaries are ahead. In all states which acknowledge human rights, the rights and interests of all handicapped groups and individuals must be respected, and all types of handicaps must be treated. If the legal inviolability of inherent human rights remains assured, it would be in the best interest of handicapped groups, and of everybody else, to determine the causes of intellectual malfunction. Even granting that various findings may create political complications, the causes and consequences of low capacity must be regarded as a problem that should command top priority. To be sure, the frequency of low intelligence may *not* be growing; but it is too high already. The low capacity people need help that must be provided to the best of our expanding scientific ability.

Thus, according to all canons of academic freedom, the causes of intellectual malfunction should be analyzed, for the purpose of

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overcoming mankind's outstanding deficiency. If the delicate difficulties that are involved were handled objectively and tactfully, for example by a prestigious body like the Academy of Science, trouble could be avoided and broadly based support for the project could be mobilized.

* * *

But Shockley is blocked—even in his own university. In their explanations his opponents frankly invoke political reasons, *e.g.* " climate of the country " and the " possibility of . . . disruptions." Fair enough. But seeking refuge behind " political climate " is a traditional technique of circumventing academic freedom.

A second argument was that Shockley, except for statistics, was not " competent " in genetics, biology, and other related sciences. How was this determined? Well, Shockley enjoyed no formal instruction in the relevant fields—in other words, he is self-taught. Actually, this is not entirely correct. Be that as it may, discovery, innovation, and pioneering unavoidably are predicated *on self-instruction. Every mature scientist teaches himself,* and does so effectively—this is the chief capability that distinguishes him from a student.

Note that subjects which involve several diverse disciplines do not usually have a firm structure and a clearly delineated scope of competence. They are often handled by persons who must fill the gaps in their knowledge as they go along—a routine that was commonly practiced by American universities faced by student unrest and by demands for new courses.

The criterion that a man who was capable of earning a Nobel Prize, also needs formal instruction at the undergraduate and graduate levels before he is able to become competent in new subjects is hilarious. Einstein was right when he advised Velikovsky to take such arguments from their comical side. Nevertheless, arguments about the competence of a first-rate scholar in a field which he explored for years suggest megalomania or *mala fides.*

Truth or untruth are not determined by democratic votes of scholars, even if they are entirely *bona fide,* let alone by autocratic over-ruling of democratic votes.

To silence a scientist by eliciting a consensus from scientists who are hostile to him, is the classic method of killing academic freedom.

Since the times of Aristotle, truth and untruth should *not* be decided by the authority of men of " stature " or " international repute," nor by mere assertions that the non-conformist made no " adequate response " to the " stringent criticism " that was addressed to him. (Such language is reminiscent of the Lysenko

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controversy in the USSR.) The essential point is that academic freedom is *inherent* in the academic as a *person,* and in his scholarly activities, and is *not* subject to vote or consent by other academics. Stature, repute, and authority are elements of subjectivity, *not* objectivity.

In the present case, there was a lot of disapproval and indignation but no serious criticism. The vagueness of the objections—and the pitifully weak argument of the verdict against Shockley—must amaze any objective observer.

It was, for that matter, entirely unscientific when the judges—"of stature"—based their verdict on ignorance or misunderstanding of what Shockley had been saying. It was downright mischievous, to choose a nice term, to assert that what Shockley proposed "seems to be genocidal policies." If it "seems" that way, why was not the doubt clarified before such a deadly accusation was made in public?

After asserting that the university has an "obligation to encourage . . . heterodox 'dangerous' thoughts," the Dean laid down his ruling, in the best Louis XIV style: "I will not authorize the course." But as a modern permissivist, he declared that if Shockley wanted to give his lectures anyway (*i.e.* without listing them in the catalogue and without credit to the students), he "may do so without special permission from anyone." In other words, Shockley is allowed to speak. But no student should listen to him.

Academic freedom is the most forward frontier against totalitarianism.

Alas, modern universities defend this frontier badly.

Meanwhile, human intelligence, which is mankind's most critical resource, is not given the scientific care it requires.

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The Ethnical Basis of the Structure of the Italians: Part I

By MARIO CAPPIERI

FORWARD

The stature, though it be a character influenced in some measure by social and environmental factors, has a genetic basis. When the stature of a population is considered during a long period of time its data are sufficient to outline the trend of this character. In Italy the only statural data available in reasonable quantity for studying the phenomena are those taken by military doctors at the examination of recruits. It is true that these data are only " samples," because they refer only to males and do not indicate the maximum height of the individuals, but as we have no other means of knowing the value of the Italian stature, we must therefore remember the Latin proverb *Non omnia possumus omnes*.

These data were published each year in the *Statistical Yearbook* up to 1963 and I availed myself of the data for 1963 which refer to recruits born in 1938. It is obvious that a statural average of the Italian population based on these data represents only a statistical calculation. It does not provide knowledge of the phenomenon in itself because the ethnical composition of the Italians is such that the stature must be studied locally. The stature varies very much, from north to south of the peninsula, and in the large islands, and it follows the different primary " ethnias." I call them ethnias and not " sub-races " or " varieties " because the racial question is controversial.

A large part of the Italians belongs to the Mediterranean race, but a certain caution is required in the attribution of some other parts. The value of the Italian stature follows well the regional differences; these have a genetical basis and therefore reveal real genetical differences of the various ethnical groups, notwithstanding the many influences to which stature was, and is subjected during so many centuries. It should be pointed out that after the Second World War a large number of southern Italians took up residence in northern Italy, altering therefore its statural aspect. But, as my study is based on the data of subjects born in 1938, the point, even partially, is excluded in my considerations. My study is a local analysis, and it must lie on very ancient basis, even before the Roman predominance in our peninsula.

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We must therefore study the aspect in its ethnical differences which are presumably in such a time and which have been transmitted through generations and centuries. Before the raising of Rome Italy's population was scarce and divided into many tribes, which were fighting each other. Due to the mountainous nature of central and southern Italy, the tribes lived in very isolated communities with their own customs and rules and were strictly endogamous. Endogamy acted in the small populations as a restriction to the variability of the phenotypical characteristics, one of which is stature. Thus it is obvious that the demographic size of these settlements exerted a considerable influence on the process of their somatic evolution, even as slight as we can surmise. The present stature reflects the ancient position of the ethnical groups to which the Italian population belongs.

I want to dispel a belief which is largely ingrained in people: that there were in Italy during the course of time large movements of population and/or deep interbreeding between the ethnias. Without denying it on the whole, we must resist it because, in the Metal Ages, the population had left the precarious life of the Neolithic and was organising permanent settlements and rural communities. The stability *in loco* of the various archaic tribes constitutes, even partially, a factor of undoubtedly genetic homogeneity during the course of time.

The statural variety of the Italians shows similar, or nearly similar, differentiations in the ethnias that I have schemed, because their character, being basically genetic, has preserved its essential aspect through the evolution of the centuries. I assume, therefore, that the statural variations of the present population indicated, *grosso modo*, real differentiation in the genotype of the population. It is my intention to analyse whether, and up to which point, these differences fit my scheme of the Italian ethnias.

THE ETHNIAS OF THE ITALIAN POPULATION

I have identified the following ethnias in the Italian population:—

(a) the Celtic, divided into two varieties: the Gallic and the Ligure;

(b) the Veneti, divided into two varieties: the Euganean and the Illyrian;

(c) the Etruscan;

(d) the Japygean;

(e) the Italic, divided into three groups of "peoples": 1. Ombreans; 2. Samnites; 3. Sabelleans.

(f) the Sicula;

(g) the Sardinian.

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The ancient historians also considered distinctly these populations, which had their own customs, uses and languages. They called them " peoples," sometimes *stirpi* (stocks), and some were divided into " tribes." These historians represented them as bound by so-called " blood-links," a term obviously equated with our " genetical relation."

I have based my discriminant criterion on various elements: (a) the study of all the historical sources; (b) the examination of the beliefs about the local ethnical origin; (c) the analysis and comparison of the somatic characteristics, noted and described by Livi in the two volumes of his *Antropometrica militare* (Ministero della Guerra, Roma, 1898 and 1905) on the recruits of nearly a century ago. Such an analysis has a particular importance in regard to the provincial populations which reside at the boundaries of the ethnias, and are apt to represent a mixed population in a " territory of confluence."

Because the stature is influenced by social and ecological factors, I have taken account of them in discussing the results. It is still a subject of vivid discussion the value of these factors set against the genetical factors in the human evolution and in the evolution of the characters. In fact, we cannot judge in abstract but only in relation to certain concrete environmental differences, and to certain hereditary concrete differences, particularly when we consider stature. In effect, for a population in which all the individuals were in possession of a similar hereditary patrimony, the statural differences of the individuals of the successive generations would depend, at the most, on the environment, while when all the individuals would live in analogous environment, nearly all the differences depend on their heredity. Now, the stature varies very largely, following the uniformity or diversity of the environment in which the population live, and the social and alimentary customs they have. I have assumed that within the same genotype, or at least their varieties (1) the coastal populations are taller than those living inland; (2) the isolated populations, all the other conditions being similar, have a minor stature; (3) in general the population of the chief town of a province or of an ethnia affects the stature of the population of these aggregations where it lies, showing higher stature than that of the population of other provinces.

I have outlined in Map 1 the presumable aspect of the primitive settlements existing in archaic times in Italy. The population was scarce, occupying large territories of plains or mountains and divided into some large ethnias:—

(1) the Ligure, which lived in the present regions of Piedmont, Aosta, Liguria, and Lombardy up to the Ticino river; southward it reached the Magra river on the Tyrrhenian coast;

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(2) the Etruscan, whose settlements were in the present regions of Lombardy, partially in Veneto up to the Adige river, in Aemilia-Romagna, Tuscany, part of Umbria; Latium, from the Magra river on the Tyrrhenian coast and from the Adige river on the Adriatic coast to the Tiber to the south; in the east the Etruscans reached the Savio river, a little to the south of Ravenna;

(3) the Veneti, whose settlements were in the north-east of continental Italy, that is in the present region of Veneto, from the Adige river to the Gail river towards the Karavanken mountains;

(4) the Ombro-Sabellean, which lived in the rest of the peninsula and southern Italy, except for the present region of Apulia, where lay the settlements of another ethnia;

(5) the Japygean;

(6) the Sicula;

(7) the Sardinian.

But already in historical times, in the first half of the first millennium B.C. a large spreading of the Celts took place in continental Italy, and their pressure compelled the Ligures to retreat and concentrate in the arc of the Ligustic gulf. The Gauls submerged the scarce population of the Padan valley and of the Aemilian region. The interbreeding of Gallic-Ligurian tribes, in the southern Piedmont, does not permit any discriminant analysis because the Gauls and Ligures were two varieties of Celts, genetically very closely related. Very different is the case of the Gallic-Etruscan cross-breeding, whose genetic origins were clearly distinct. But such inter-breeding was evidently modest, because the scarce Etruscan population in the Padan valley and in the Aemilian region was rapidly assimilated by the very much more numerous Gallic population. As a consequence, by the middle of the first millennium B.C., the racial aspect of continental Italy was totally different from that which I have outlined in Map 1. In fact, by now the Gauls and the Ligures occupied the western and central territory of continental Italy up to the Mincio river and also all the territory of the southern Padan valley—the present region of Aemilia-Romagna up to the Apennine mountains where the Etruscans lived.

CELTIC ETHNIA

The Celts—the form derives from the Greek *Koltoi*—formed, in the Iron Age, a "secondary race" or "sub-race" divided into a certain number of populations, which, apart from the possession of the same genotype, were bound together by a common Indo-European culture and a fundamental glottological unity. These populations were: (1) the Ligurian; (2) the Celtiberian; (3) the

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MAP 1
Ethnical populations of Italy in the Iron Age.

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Gallic; (4) the Britonian; (5) the Belgian; (6) the Scotian; (7) the Pittian; (8) the Caledonian; (9) the Welsh; (10) the Britannian; (11) the Hibernian; and (12) the Galatian of Anatolia.

The Gauls had a very important rôle in the peopling of continental Italy, because in the first half of the first millennium B.C. they spread slowly in the regions along the River Po, and eastward up to the Mincio river, regions which were in part sparsely inhabited by the Etruscans, who were rapidly assimilated. In Piedmont, the Gauls met the sparse Ligure population, which was partially absorbed and partially pushed forward towards the Ligurian Apennines. The Gauls were renowned as warriors and horsemen; they were impetuous, skilled and intelligent workers.

I have distinguished the Gauls of continental Italy into two large complexes—the Transpadan Gauls and the Cispadan Gauls —because the ancient writers kept this division and because I could analyse better the intercrossing in some territories of ethnical confluence," where it is evident that there is contact between them and the Venetis, the Etruscans and the Ombreans. As I have outlined in Map 2, their settlements were located more or less in the present regions of Piedmont (partially), Aosta, Lombardy (partially), and Aemilia-Romagna, where the Gauls were divided into a certain number of large tribes all independent of each other but which, if they had a serious enough motive, could get together under a mutual agreement and act as a unified body, under decree of a general assembly of all free men.

THE TRANSPADAN GAULS

I have identified 13 tribes among these Gauls: Maielles, Medulles, Taurines, Salasses, Libicis, Lepontiis, Insubres, Orumboves (Orobis), Venonetes, Stoenis, Trumplinis, Cammuniis, and Cenomanes, of which I have outlined the probable territories and settlements in Map 3. In Table 1 I indicate the stature of the tribes and their provincial populations which are the presumable descendants, and some essential constants of the series.

The statural mean of these Transpadan Gauls is 1697 ± 1.06 with a S.D. of 5.6. The range of the means is 1687—1706 = 19 m.m. The field of oscillation of the theoretical mean[1] is

[1] I calculated a very useful index of dispersion, *i.e.* the oscillation-field of the theortetical mean. The narrower the field is, the more the two means—theoretical and observed—meet each other or coincide. This field is formed by adding to or subtracting from the value of the arithmetical mean the value of three times its probable error (i.e. A $\pm$ 3PE). The theoretical mean has a probability of 95 per cent to fall within these limits, and conversely the observed mean has the same probability to fall into an interval of 3PE over or under the theoretical mean. I have called this field, for the sake of brevity, delta (Δ).

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MAP 2

Ethnias in Italy at the time of the foundation of Rome (about B.C. 750).

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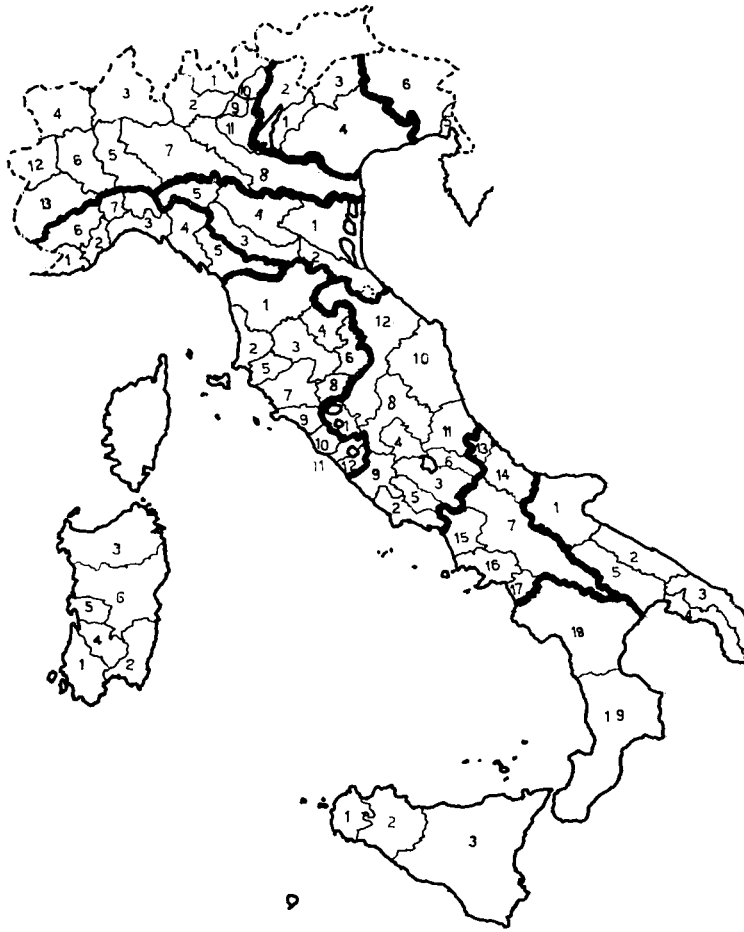

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1694—1700 = 6 m.m. This last value is indeed excellent because it means that the theoretical statural mean, at the apex of the normal curve, traced in function of the actual frequencies, falls into a brief field of 6 m.m.—3 m.m. over and 3 m.m. under the actual mean. The central value of this group falls among the population of Aosta (1699) in the Salasses tribe. In order to obtain the statural homogeneity of this group, we must analyse the values of the S.D.s. It is well known that the normal curve has two essential paramenters, arithmetic mean (A) and S.D. (σ), which represent, respectively, the tendency towards the centre of the series and the degree of the dispersion of the items of the series. The former is the value of the mean and the latter of its variability which shows the degree of the density of the serial terms. Therefore a high value of the S.D. in relation, of course, to its inter-racial limits, is an indication of a notable dispersion of the individual values. There is a strict relation between the expanse of the series (N) and its S.D., because the greater the former, the more actual the value of the latter. But we know that in the case of biological characteristics (and stature is one) the possibility that a value transgresses the amount of three times the S.D. of the series is highly improbable. Therefore, a value which falls into the limits of the mean plus or less one S.D. (*i.e.* $A \pm 1\sigma$) has a maximum

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MAP 3

(opposite page)

The probable localisation of the tribes, "peoples," "nations," and *stirpi* at the time of the foundation of Rome (about B.C. 750).

Tribes of the Transpadan Gauls: 1 Venonetes; 2 Orumboves (Orobis); 3 Lepontiis; 4 Salasses; 5 Libicis; 6 Taurines; 7 Insubres; 8 Cenomanes; 9 Cammuniis; 10 Stoenis; 11 Trumplinis; 12 Medulles; 13 Maielles.

Tribes of the Cispadan Gauls: 1 Lingones; 2 Senones; 3 Friniates; 4 Boiis; 5 Anamares.

Tribes of the Ligures: 1 Intimiliis; 2 Ingaunis; 3 Veitures; 4 Velleiates; 5 Apuanes; 6 Bagiannis; 7 Casmonates; 8 Celelates.

Stirpi of the Venetis: 1 Bellunis; 2 Anaunis; 3 Catubrinis; 4 Euganis; 5 Histrians; 6 Carnicans.

"Lucumonies" of the Etruscans: 1 Volaterrae; 2 Populonia; 3 Clusium; 4 Arretium; 5 Vetulonia; 6 Perusia; 7 Rusellae; 8 Volsinii; 9 Vulci; 10 Tarquinii; 11 Caere; 12 Veii.

Japygean "peoples": 1 Dauniis; 2 Peucetiis; 3 Messapiis; 4 Salentineans; 5 Poediculis.

Italic "nations" and *stirpi*: 1 Falisqueans; 2 Volsqueans; 3 Marsicans; 4 Aequeans; 5 Herniceans; 6 Paeligneans; 7 Samnitis; 8 Sabineans; 9 Latins; 10 Piceneans; 11 Vestineans; 12 Ombres; 13 Marrucineans; 14 Fraentaneans; 15 Auruncis; 16 Opiceans; 17 Picentineans; 18 Lucaneans; 19 Bruttiis.

Sicilian *stirpi*: 1 Elymeans; 2 Sicans; 3 Siculis.

Sardinian tribes: 1 Sulcitans; 2 Siculensis; 3 Corsicans; 4 Scapitans; 5 Celsitans; 6 Barbariceans.

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TABLE 1

Stature in m.m. and some constants of tribes and provincial populations of the Transpadan Gauls

Tribes	*Provincial populations*	*Stature in m.m. Prov.*	*Tribe*	±	*S.D.*
Salasses	Aostanis	1699	1699	—	—
Medulles	Torinesis	1701	1701	—	—
Venonetes	Sondrinesis	1692	1692	—	—
Stoenis	Brescianis	1687	1687	—	—
Maielles	Torinesis	1701			
	Cuneensis	1704	1703	1.06	2.2
Taurines	Torinesis	1701			
	Astigianis	1702	1702	0.48	1.0
Libicis	Vercellesis	1702			
	Alessandrinis	1709	1706	2.40	5.0
Orumboves	Comaschis	1695			
(Orobis)	Bergamaschis	1691	1693	1.34	2.8
Cammuniis	Brescianis	1687			
	Bergamaschis	1691	1689	1.34	2.8
Trumplinis	Brescianis	1687			
	Bergamaschis	1691	1689	1.34	2.8
Lepontiis	Novaresis	1697			
	Vercellesis	1702			
	Varesinis	1701			
	Comaschis	1695	1699	1.12	3.3
Insubres	Novaresis	1697			
	Varesinis	1701			
	Comaschis	1695			
	Milanesis	1707			
	Pavesis	1696			
	Cremonesis	1696	1699	1.29	4.6
Cenomanes	Bergamaschis	1691			
	Brescianis	1687			
	Mantovanis	1696			
	Veronesis	1704			
	Padovanis	1706			
	Venezianis	1712			
	Rodiginis	1689	1698	2.40	9.6

of probability to belong to the same series.[2] In the present instance this field of $A \pm 1\ \sigma$ is 1691—1703 = 12 m.m.; nine of the 13 means fall into it, and the other four into the limits of $A \pm 2\sigma$. That indicates a modest dispersion of the tribal means.

I have then calculated the theoretical values of the normal curve on which the actual statural values could fit the most. I traced in consequence the normal curve with the essential elements,

[2] The criterion of Sigma (σ) derives from (1) the concept that the curve of the values of the characters of the homogeneous biological units is generally normal or nearly normal, (2) in this case the area under the curve represents probability, (3) 99.73 per cent of this area falls between the values of three times the S.D. (or Sigma) of the arithmetical mean of the series. By convention, therefore, it is assumed that a given value or sample belongs to the series when it falls within its mean $\pm 3\ \sigma$.

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i.e. A 1699, N 20, S.D. 6.9; the theoretical curve presents n 10, χ^2 8.2, from which the value of P is 0.61. Such a high level of probability indicates an excellent reciprocal fitness of the two curves. From all these elements I assume that my scheme of the ethnical ancestors of the present Celts in regard to the stature is correct and answers well to many biometric trials. I assume also that the present values of the stature of these presumable Celts reflect the ancient ethnia and consequently that the genetic factors are the same, at least for the stature.

I have calculated the mutual differences of means of the 13 tribes with t-test (limit of P = 0.05)[3] and the result is that 87.3 per cent of the differences are "non-significant." These reciprocal differences of means are indicated in Table 2.

TABLE 2

Reciprocal statural differences in m.m. of the means of the tribes of Transpadan Gauls

Tribes	*Sal.*	*Med.*	*Ven.*	*Sto.*	*Mai.*	*Tau.*	*Lib.*	*Oro.*	*Cam.*	*Tru.*	*Lep.*	*Ins.*	*Cen.*
Salasses	—	2	7	12	4	3	7	6	10	10	0	0	1
Medulles	—	—	9	14	2	1	5	8	12	12	2	2	3
Venonetes	—	—	—	5	11	10	14	1	3	3	7	7	6
Stoenis	—	—	—	—	16	15	19	6	2	2	12	12	11
Maielles	—	—	—	—	—	1	3	10	14	14	4	4	5
Taurines	—	—	—	—	—	—	4	9	13	13	3	3	4
Libicis	—	—	—	—	—	—	—	13	17	17	7	7	8
Oromboves	—	—	—	—	—	—	—	—	4	4	6	6	5
Cammuniis	—	—	—	—	—	—	—	—	—	0	10	10	9
Trumplinis	—	—	—	—	—	—	—	—	—	—	10	10	9
Lepontiis	—	—	—	—	—	—	—	—	—	—	—	0	1
Insubres	—	—	—	—	—	—	—	—	—	—	—	—	1
Cenomanes	—	—	—	—	—	—	—	—	—	—	—	—	—

These reciprocal differences may be imagined as points inscribed on a circular area on which the single statural means indicate their mutual correct position, assuming therefore the real

[3] With Student's t-test (which derives from the "small samples theory") we can ascertain whether a difference between the two means is real or casual. The t-values indicate the degree of probability with which the casual values can produce a difference analogous to that we have found. We assume, by convention, that a difference be "significant" when the value of the said probability reaches the figure of 5/100 or 0.05. The notations are as follows:

$d = A_1 - A_2$ = difference of means

$p = (N_1 - 1) + (N_2 - 1)$ = degrees of freedom

$$b = \sqrt{\frac{1}{N_1} + \frac{1}{N_2}}$$

$c = q/p \qquad f = c^{1/2} \qquad t = \frac{d}{f - b}$

p = sample sum of squares of A_1 + sample sum of squares of A_2

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distance in function of the reciprocal values from the central value of the ethnia. Each mutual difference shows not only the centrifugal tendency from that mean, but also that of the other tribes. The synthesis of all these positions, which sums up the whole centrifugal tendency of the totality of the tribes, is represented by the inter-tribal index of variability[4] whose range is 0.0—20.0 (for these values) and whose value in the present case is 7.1.

Each tribe has its own variability shown by its S.D. which may be considered as the intra-tribal index of variability. Such a value is rather modest in all the tribes (excepting the Cenomanes) with a mean of 3.4. The values of the first four tribes in Table 1 do not allow any calculations, being single values. I have therefore made with them a series with the result of a statural mean of 1695 ± 2.21 and S.D. of 6.5. The mean of all these tribal deviations from the central value is 4.1 m.m. The value of the Cenomanes is rather high—9.6—in comparison with those of the other tribes, but I explain it by the fact that such a tribe inhabited the most eastern part of the territory of the Transpadan Gauls, an "area of confluence" where evidently occurred some contact with the Euganeans, and consequently a process of interbreeding.

The tallest statures are shown—even partially—by the tribes of Piedmont. Having analysed these values, I identified a group of tribes—Libicis, Maielles, Taurines, Medulles, Lepontiis, Salasses, and Insubres—with a mean stature of 1701 and S.D. of 2.6, and a second group of tribes of a minor stature—mean 1691 and S.D. of 3.6. These two groups are separated, more or less, by an ideal slanting line stretching from Como to Colorno on the River Po. The difference of one centimetere between the two means could be ascribed to both genetical and environmental influence; the former is negligible, due maybe to a prehistoric intercrossing of these western Gauls with the Etruscans, a fundamental but rather scarce population of Piedmont and Lombardy before the Gauls' invasion. The latter is represented by the "chief town influence" (Alessandrinis 1709, Astigianis 1702, Torinesis 1701).

I have then formed a single series with the statural values of

[4] From the "sample variance" $V = \sigma^2 = 1/N \Sigma (x-x)^2$ we obtain the population variance by dividing the sum of squares (*i.e.* $\Sigma(x-x)^2$ by the number of degrees of freedom (*i.e.*, $N-1$), and thus $V = \frac{(x-x)^2}{N-1}$. The scheme of results is as follows:—

Source of variability	*Sum of squares*	*Degrees of freedom*	*Variance estimate*
Within samples, *i.e.* intra-group variability	a	b	a/b = w
Between samples, *i.e.* inter-group variability	c	d	c/d = z

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the provincial populations which were previously scattered in the proper tribes of my scheme. This series has a mean of 1697 ± 0.79 and S.D. of 6.6. While the average stature is the same as in the tribal series, the values of the P.E. and of the S.D. are a little different, because the basis of the calculation is different. In general, if the number of terms (N) is large the values of these parameters are nearer to actual values. In fact, the correct and actual values of the essential parameters—arithmetic mean (A), probable error (P.E.), and standard deviation (S.D. or σ)—are in direct ratio of the number of items (N) taken into calculation; therefore, the larger is N, the more actual are P.D. and S.D.

This provincial series has a range of 1687—1712 = 25 m.m., and a delta field of 1695—1699 = 4 m.m. Twenty-four of the 33 values fall in the limits A ± 1 σ, eight in the limits A ± 2 σ and one in the limits of A ± 3 σ. The index of interprovincial index of variability, based on 528 reciprocal differences of means, gives a value of 7.8. The mean of these provincial deviations from the central value is 5.5 m.m. As I have already observed, the highest statures are found among the greater part of the Piedmont population, while the smaller stature is found specially among the Lombardy population. I have also observed that the stature is rather influenced by social and environmental factors, particularly evident in modern times.

Now a question: is it possible to discriminate the stature affected by these factors from that which we may assume as free from these factors, and which, for short, I call "original"? It is a very difficult problem because the three components are extremely uncertain and liable to controversy. To approach this problem I assume (1) that social factors affect the "chief town population" with an influence which is sometimes independent and sometimes convergent; (2) that the environmental factors affect the "coastal population" and the "chief town population." Whence it follows that, in a very tentative way, if I eliminate from the series the items representing populations affected by "coastal, chief town and isolation" influences (which I call "affected") there remains the population which presumably represents the "original" population. Trying this discrimination on the Transpadan Gauls I ascribe 42.5 per cent to the part of the population which seems not affected by social and environmental factors, another 36.3 per cent to the part of the population termed by me "affected," and the last 21.2 per cent to the population of the "area of confluence" or "intercrossed" population—the Gauls, Euganeans, Helvetiis and Raetiis. The first group shows a mean stature of 1693 ± 0.67 and S.D. of 3.7; the second group a mean of 1703 ± 0.53 and S.D. of 2.8; the third group a mean of 1698 ± 2.40 and S.D. of 9.6. The S.D. of this third (interbred)

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group indicates by its level —9.6—the fundamental heterogeneity. After cautious calculation I notice that the difference of mean between the value of the "non-affected" population and the "affected" one is 10 m.m. I assume it to be the weight of the social and environmental factors on the "original" population; it represents 0.6 per cent of the statural mean of "non-affected" stature, a value indeed trivial in this calculation.

Tentatively I assume that a fifth of these Transpadan Gauls represents the result of interbreeding with (1) the Euganeans, precisely in the territory of the Cenomanes tribe, which is intersected by the rivers Mincio, Adige, Adigetto and lower Bacchiglione, and (2) a quota impossible to ascertain of prehistoric interbreeding with the primary Etruscans of Piedmont and Lombardy; and (3) and some with Helvetiis and Raetiis. I do not take into consideration the inevitable prehistoric assimilation of the Ligures by the Gauls in southern Piedmont, which is not now absolutely identifiable.

I have calculated—for statistical purposes—the mean stature of the population of the administrative regions of Piedmont and Lombardy, whose respective means are 1702 ± 0.98 with a S.D. of 3.9 and 1696 ± 1.28 with a S.D. of 5.8.

THE CISPADAN GAULS

This subdivision of the Gauls has not a single position in a genetic sense because while they were of the same strain of the Gauls inhabiting the territories over the Po river, they came into contact with populations of another genetical stock—Etruscans and Ombreans, whose influence must be considered. The ancient writers kept them separated and I do the same because it is more convenient for my analyses of the data in order to ascertain the degree of intercrossing with other ethnias.

I distinguished five tribes of these Gauls: Anamares, Friniates, Senones, Lingones, and Boiis, for which I indicate the statural means and some essential constants of the presumable descendants in Table 3.

The statural mean of this tribal complex is 1703 ± 1.80 with a S.D. of 2.9. The range of the terms is 1698—1705 = 7 m.m.; its delta-field is 1698—1708 = 10 m.m. Four of the five tribal means fall within the limits of $A \pm 1\sigma$, and one within the limits of $A \pm 3\sigma$. I have calculated the reciprocal difference of means and the values in Table 4.

All these differences, checked with the t-test, are "non-significant." The index of the inter-tribes' variability is 3.2. The central mean falls in the Boiis tribe within the Parmense population. The mean of the tribal deviations from the central value is 2.0 m.m. The influence of the "chief town" factor is evident: the stature of

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TABLE 3

Stature in m.m. of tribes and provincial populations of the Cispadan Gauls

Tribes	*Provincial populations*	*Stature in m.m.* *Provin.*	*Tribe*	±	*S.D.*
Anamares	Pavesis	1696			
	Piacentinis	1700	1698	1.34	2.8
Senones	Bolognesis	1711			
	Ravennatis	1705			
	Forlivesis	1695	1704	3.16	8.1
Lingones	Bolognesis	1711			
	Ravennatis	1705			
	Ferraresis	1700	1705	2.18	5.6
Boiis	Parmensis	1703			
	Reggianis	1705			
	Modenesis	1701	1703	0.62	1.6
Friniates	Parmensis	1703			
	Reggianis	1705			
	Modenesis	1701			
	Bolognesis	1711	1705	1.50	4.4

TABLE 4

Reciprocal differences in m.m. of the tribal means of the tribes of Cispadan Gauls

Tribes	*Anamares*	*Friniates*	*Senones*	*Lingones*	*Boiis*
Anamares	—	7	6	7	5
Friniates	—	—	1	0	2
Senones	—	—	—	1	1
Lingones	—	—	—	—	2
Boiis	—	—	—	—	—

the Bolognesi—1711—is much higher than the mean—1701—of the others pooled together. So the Lingones tribe appears to be the highest of the five tribes, while the Anamares show the smaller —1698—stature. This tribe is the most western, positioned at the limits of the ethnia, and presents some characteristics of isolation, situated in a poor area on the south bank of the Po river. It is symptomatic that the Transpadan tribes across the river have a statural mean of 1697, practically the same as the Anamares.

Now I have considered the provincial populations, regardless of the tribes which compose the whole of the complex. The series of the 15 populations gives a mean stature of 1703 ± 0.85 and S.D. of 5.0. The range of the terms is 1695—1711 = 16 m.m.; its delta-field is 1700—1706 = 6 m.m. Of the total of 15 values, ten fall within the limits of A ± 1 σ; and five into the limits of A ± 2 σ. The index of interprovincial variability is 5.7. The mean of the deviations of the provinical values from the central mean is 3.8 m.m. Among these provincial statures the highest—1711—is presented by the Bolognesis, and the smaller by the Forlivesis. The territory of these Forlivesis is rather an "area of confluence" where the Ombreans represented the Italic element. As already

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mentioned, the Bolognesis show the highest stature—1711—due also to a " chief town " influence. The calculation made with the provincial populations shows a larger deviation—mean 5.7—than that made employing the tribal values—3.2—but of course that is because the former is based on a different calculation. The highest deviation from the central value is that of the Bolognesis and Forlivesis with 8 m.m., followed by the Pavesis with 7 m.m.; all the others show a modest deviation—2 or 3 m.m.

Also with this complex I tried to differentiate the social, environmental, intercrossing factors from what I call the " original " aspect of the population. With caution I assume that the 41.2 per cent represents the " original " population, the same figure represents the population " affected," and the 17.6 per cent represents the interbred population. The first group shows a stature of 1701 $\pm$ 0.60 and S.D. of 2.4; the second a mean stature of 1708 $\pm$ 0.70 and S.D. of 2.8: the difference between these two means is 7 m.m., which is the 0.4 per cent of the " original " population. The third group shows a stature of 1704 $\pm$ 3.16 and S.D. of 8.1. This rather high figure shows a certain heterogeneity due to the probable intercrossing of the Senones tribe with the Ombreans.

I calculated the parameters of the normal curve which could better meet the actual curve. The result is rather satisfactory, because through the following values A 1703, N 15, S.D. 5.0, n 8, P 0.09, I got a curve within the limits of " non-significant," noting that I consider this field up to P = 0.05. I calculated also the stature of the population within the administrative limits of the present Aemilia-Romagna region, *i.e.* 1703 $\pm$ 1.15 with a S.D. of 4.8.

ANALYSIS AND DISCUSSION ON THE GAULS' DATA

I have assumed that the Gauls formed their own ethnical entity in the Padan valley and the statural data confirm this thesis. For the 18 tribes I obtained a statural mean of 1698 $\pm$ 0.96 with a S.D. of 6.0; the range of items is 1687—1706 = 19 m.m.; the delta-field is 1695—1701 = 6 m.m. Of the 18 means 12 fall into the limits of A $\pm$ 1 σ and six into the limits of A $\pm$ 2 σ.

Calculating then the provincial values, I obtained a statural mean of 1699 $\pm$ 0.67 and S.D. of 6.7; the range of the items is 1887—1712 = 25 m.m.; its delta-field is 1697—1701 = 4 m.m. Of the 48 means, 33 fall within the limits of A $\pm$ 1 σ and 15 into the limits of A $\pm$ 2 σ. I have calculated the parameters for the normal curve with the data of the provincial values through the following elements: A 1700, N 26, S.D. 6.5, n 14, obtaining x^2 10.2 with P 0.12, a result indeed very satisfactory because the approximated normal curve (Graph 1) fits well enough the actual

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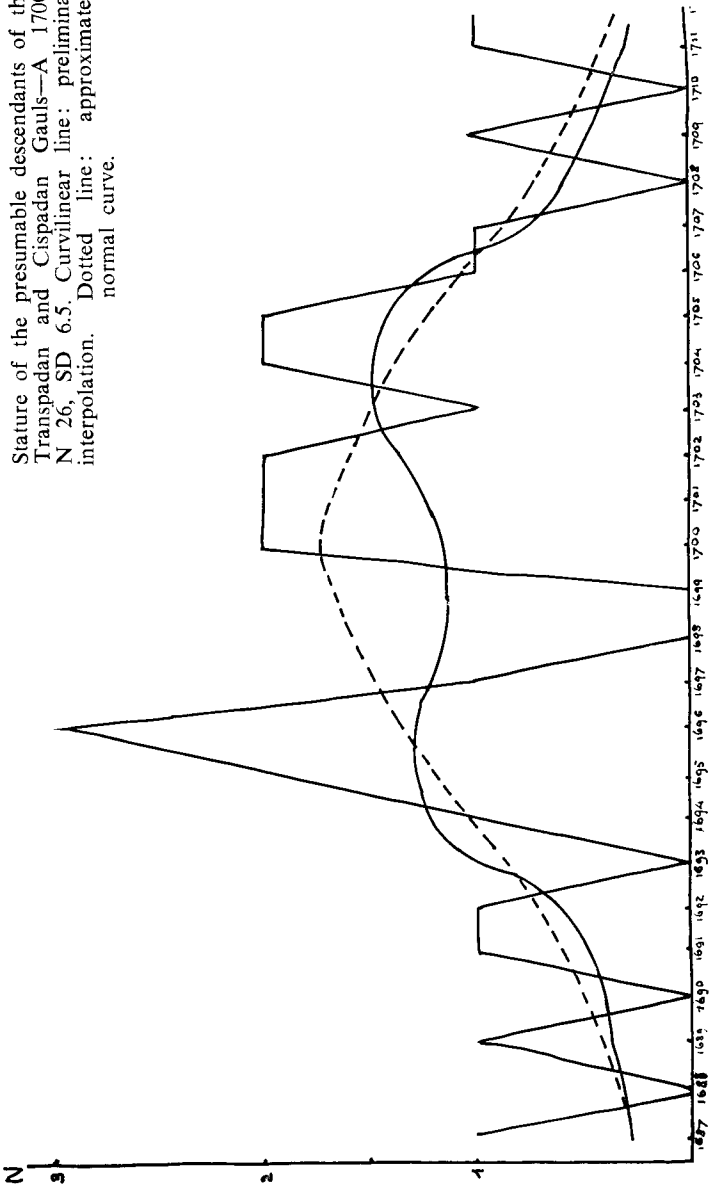
GRAPH 1

Stature of the presumable descendants of the Transpadan and Cispadan Gauls—A 1700, N 26, SD 6.5. Curvilinear line: preliminar interpolation. Dotted line: approximated normal curve.



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curve of distribution. These values are in agreement with the value of the oscillation field of the theoretical mean—4 m.m.; in other words that means that the theoretical means fall within the limits of 4 m.m. in the curve of distribution of the actual values, *viz.* 1697—1701.

THE LIGURES

The Ligures formed a variety of the Celtic type. In prehistoric times they inhabited the north-western part of continental Italy. The expansion of the Gauls in the late Iron Age pushed the Ligures into more restricted areas, particularly towards the Ligustic arc. At the time of the Punic wars, they occupied territory much larger than that known as the Ligurian region, and which I have outlined in Map 2. The historical sources concerning the Ligures are few. This population inhabited hilly or mountainous territories and had a reputation of having a warlike nature.

I have distinguished eight tribes among the Ligures and in Table 5 I indicate the stature of their presumable descendants, and

TABLE 5

Stature in m.m. and some constants of the tribal and provincial populations of the Ligures

Tribes	*Provincial populations*	*Stature in m.m.* *Provin.*	*Tribal*	±	*S.D.*
Intimiliis	Imperiesis	1703	1703	—	—
Ingaunis	Savonesis	1711	1711	—	—
Bagiennis	Cuneensis	1704	1704	—	—
Celelates	Alessandrinis	1709	1709	—	—
Casmonates	Astigianis	1702			
	Alessandrinis	1709	1706	2.40	5.0
Veituris	Savonesis	1711			
	Genovesis	1713	1712	0.62	1.3
Velleiates	Piacentinis	1700			
	Parmensis	1703			
	Genovesis	1713			
	Spezzinis	1712	1707	2.21	6.5
Apuanes	Spezzinis	1712			
	Massesis	1702			
	Lucchesis	1713			
	Pistoiesis	1718	1711	2.28	6.7

some constants. These tribes are: Intimiliis, Ingaunis, Bagiennis, Celelates, Casmonates, Veituris, Velleiates, Apuanes.

The mean stature is 1708 ± 0.84 with a S.D. of 3.5; the range of the terms is 1703—1712 = 9 m.m.; the delta-field is 1705—1711 = 6 m.m. Six of the tribal means fall into the limits A ± 1σ, the other two into the limits A ± 2σ. The mean of the tribal means deviation from the central value is 3 m.m.

I calculated the mutual differences of means and indicate their

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value in Table 6. The index of intertribal variability is 4.1 which together with the intratribes index—3.5—show a good statural homogeneity. I have checked these 22 differences of means with the t-test and ascertained that all are "non-significant." These descendants of the prehistoric Ligures show high stature, to which

TABLE 6

Reciprocal differences in m.m. of the tribal means of the Ligures

Tribes	*Int.*	*Ing.*	*Veit.*	*Vell.*	*Apua.*	*Bag.*	*Casm.*	*Cele.*
Intimiliis	—	8	9	4	8	1	3	6
Ingauniis	—	—	1	4	0	7	5	2
Veituris	—	—	—	5	1	8	6	3
Vellelates	—	—	—	—	4	3	1	2
Apuanes	—	—	—	—	—	7	5	2
Bagiennis	—	—	—	—	—	—	2	5
Casmonates	—	—	—	—	—	—	—	3
Celelates	—	—	—	—	—	—	—	—

contribute the environmental factors because the population inhabited more or less coastal territories and we know that this fact influences the stature. The high statures are also influenced by the "chief town" factor, *i.e.*, that the chief-town population have a statural value higher than the rest of the population of the province—Genovesi 1713. The central mean lies on a line passing along the Casmonates and Celelates tribes in the Alessandrian population. The various tribes do not show any particlar centrifugal tendency and the inter-tribal index—4.1—points to this.

Among the Ligures the environmental and social influences are predominant and parallel, the population living in hilly and mountainous regions shows lesser stature. Positive genetical influence could be surmised for the stature of the eastern tribe, the Apuanes—1711—which inhabited a region or "confluence," coming in contact with the Etruscans.

I have then compiled a series with the means of the provincial population—N = 16—which gives a mean of 1708 ± 0.92 and S.D. of 5.4; the range of the 16 terms is 1700—1718 = 18 m.m.; the delta-field is 1705—1711 = 6 m.m. Twelve of the means fall within the limits of A ± 1 σ; the other four into the limits of A ± 2 σ. The mean of all the deviations from the central value is 5 m.m. I have calculated also the reciprocal difference—120—of the provincial means and obtained an index of interprovincial variability of 6.2. I have tentatively tried to discriminate among the Ligures the three groups, *i.e.*, the "original," the "affected," and the "interbred." The values respectively are: 15.8 per cent of the whole population, statural mean 1708 ± 1.83 and S.D. of 4.7; 21.1 per cent, 1713 ± 0.34, S.D. 1.0; 63.1 per cent, 1708 ± 0.84,

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S.D. 2.8. This shows that the Ligures are deeply affected by the intercrossing with other populations, some of the same genetical stock, some of the other ethnias, *viz.*, Transpadan and Cispadan Gauls, and Etruscans. The tribes more affected by this intercrossing process are the Bagiennis, the Celelates, the Casmonates, the Velleiates and the Apuanes. The difference between the " original " and that " affected " is very small—5 m.m.—and represents 0.3 per cent of the stature of the " original " population.

I have also calculated the mean stature of the population in the limits of the administrative region of Liguria, obtaining 1710 ± 1.56 and a S.D. of 4.6.

DISCUSSION ON THE DATA OF THE CELTS IN ITALY

I have formed a series with the tribal values of stature—26—of the three varieties of Celts, obtaining a mean of 1701 ± 0.46 and S.D. of 5.7. The result is satisfactory and points to a very good statural uniformity of a very large population: about 18 million on an area of nearly 80,000 square kilometres. I assume that such uniformity is a function of a genetical homogeneity.

In the preceding pages I have indicated some percentages which I apply to the three parts of the population: (1) the population which I assume to be free from social and environmental influences, which I call for short " original "; (2) the part which I assume is influenced by social and environmental elements, and which I call for short " affected "; and (3) which I judge represents the population of mixed origin in the " territory of confluence." Summarising the data already presented I indicate the following percentages:

Varieties of Celts	*" Original "*	*" Affected "*	*Mixed origin*
Transpadan Gauls	42.5	36.3	21.2
Cispadan Gauls	41.2	41.2	17.6
Ligures	15.8	21.1	63.1

The first two varieties seem to have a similar aspect: because they are inland populations they are not subjected to " coastal influence," but by factors of which the " chief town " influence is perceived. This occurs in the case of many tribes; the Insubris are affected by the high statural value of the Milaneses, 107; the Lepontinis by the Vercellesis, 1702; the Libicis by the Alessandrinis, 1709; the Medullis by the Turinensis, 1701; the Senones, Lingones and Friniates by the Bolognesis, 1709; the Boiis by the Reggianis, 1705; while the Maiellis, Orumbovis (Orobis) and part of the Libicis reveal intercrossing with the Ligures, the Cenomanis with the Euganeans, the Anamares with the Ligures, and the Senomes with the Ombreans. The third variety—the Ligures—show the

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general high stature due to the "coastal influence." Some are influenced by the "chief town" high values, such as the Ingaunis by the Savonesis, 1711; the Veituris by the Savonesis, 1711 and by the Genovesis, 1713. All the Liguri tribes seem affected more or less by an intercrossing with the Gauls, and the Apuanean tribe with the Etruscans. The case of the Apuanis is very particular: they show the "coastal" influence, 1711, the "chief town" influence of the Spezzinis, 1712, and the result of an intercrossing process with the Etruscans (Lucchesi 1713, Pistoiesis 1718).

VENETIS ETHNIA

In the western region of Italy there was a population called Venetis by the ancient historians. It appears that the population was divided into two varieties, strongly differentiated: one the Euganean and the other which I call for short the Illyrian. We have no basis for ascertaining the original phenotype of the Venetis; the skeletal remains are very few and of dubious date and any element for comparison is missing. While the western variety (Euganean) has a well-defined typology, the eastern variety, called by me Illyrian, is an intercrossed population showing the elements of the many invasions of the Barbarians (Germans, Scandinavians, and Slavs), at the end of the Roman Empire. Anyway, it is not the Illyrian variety of the Southern Slavs but a complex of various populations, each in various degree of expansion, the largest being the Langobardian.

THE EUGANEANS

I have differentiated four Euganean "stocks" (the ancients called them *stirpi*): the Anaunis, the Catubrinis, the Bellunis, and the Euganis, for which I indicate in Table 7 the mean stature, and

TABLE 7

Stature in m.m. and some constants of Euganeans or stirpi and provincial populations

Stirpi	*Provincial populations*	*Stature in m.m.* *Stirpi*	*Provin.*	±	*S.D.*
Anaunis	Trentinis	1705	1705	—	—
Catubrinis	Trentinis	1705			
	Bellunesis	1716	1711	3.74	7.8
Bellunis	Trentinis	1705			
	Veronesis	1704	1705	0.48	1.0
Euganis	Veronesis	1704			
	Padovanis	1706			
	Trevisanis	1709			
	Vicentinis	1711			
	Venezianis	1712			
	Bellunesis	1716	1710	1.23	4.4

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some constants for each population of the presumable descendants.

The mean stature of the four stocks is 1708 ± 1.09 with a S.D. of 3.2; the range of the items and the delta-field are both 1705—1711 = 6 m.m. The means of these four *stirpi* fall into the limits of A ± 1 σ.

I calculated the reciprocal difference between all these means as indicated in Table 8 and found that the index of interstock

TABLE 8

Euganeans—Reciprocal differences in m.m. of the stirpi means

Stirpi	*Euganis*	*Bellunis*	*Anaunis*	*Catubrinis*
Euganis	—	5	5	1
Bellunis	—	0	0	6
Anaunis	—	—	—	6
Catubrinis	—	—	—	—

variability is 3.8. I checked those differences with the t-test and ascertained that all are "non-significant." The mean of all deviations values from the central value is 3 m.m. This central mean—1708—falls among the Euganis, in the Trevisan population.

The centrifugal tendency of the *stirpi* is modest—index 3.8. The means do not reveal a great environmental and social influence, excepting the factor due to "provincial chief town" (Venesianis with the stature of 1712). Undoubtedly this Euganean population shows a very good homogeneity for the statural character—a S.D. of 3.2.

I compiled a series with the 11 provincial values and obtained a mean value of 1708 ± 0.94 and S.D. of 4.7. Its range is 1704—1716 = 12 m.m.; its delta-field is 1705—1711 = 6 m.m. Of these values, nine fall within the limits A ± 1 σ, and two into A ± 2 σ. The index of interprovincial variability is 5.2. The means of these provincial deviations from the central value is 3.6 m.m.

I tried, also with the Euganeans, to discriminate the value of the difference between the "original" population and that of the population influenced by social and environmental factors. The result is: the "original" group, 54.5 per cent of the whole complex and with a statural mean of 1705 ± 0.28, S.D. 1.0; the group affected by social and environmental factors, 27.3 per cent of the complex with a statural mean of 1711 ± 0.62, S.D. 1.6; the "heterogeneous" group—Catubrini tribe—18.2 per cent of the complex with a statural mean of 1711 ± 3.74 and S.D. of 7.8. The difference of mean between the first and the second group is 6 m.m. which compared with the "original" stature—1705—represents 0.4 per cent of the same. The rather high value of the S.D. of the Catubrini tribe shows the influence of the Illyrian population in the presumable interbreeding. The data show different

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values, either as a result of the tribal calculation or of the provincial populations, but the discrepancies are due only to the different basis of the elaboration of data; the provincial means and indices are nearer to the theoretical data.

As in many other ethnias the peripheral populations show the highest difference in stature from the central mean: for instance, the Belluneses 1716, the Venitians 1712, and the Vicentinis 1711. In some cases this is due also to a degree of slight interbreeding with the populations over the fringe of their respective territories, such as in the case of the Belluneses. I opine to exclude from the original group: (1) the Belluneses, because they had some interbreeding with other ethnias; (2) the Venitians (1712), which are influenced by social factors, being a " chief town " group; and (3) the Vicentineans (1711) and the Trevisanis (1709), because they are affected by environmental factors. So that the mean value of the Euganeans may be indicated as 1705 ± 0.34, S.D. 1.2.

Then I calculated the statural mean of the population of the administrative region of Veneto regardless of stocks, and obtained a mean of 1707 ± 2.20 and S.D. of 8.8.

THE ILLYRIANS

I sorted out among the Illyrians two *stirpi* (stocks): the Carnica and the Histrian, the stature of which and some constants I indicate in Table 9.

TABLE 9

Illyrians—Stature in m.m. and some constants of stirpi and provincial populations

Stirpi	*Provincial populations*	*Stature in m.m. Provin.*	*Stirpi*	±	*S.D.*
Carnica	Bellunesis	1716			
	Pordenonisis	1727			
	Udinesis	1727			
	Gorizianis	1740	1728	3.33	9.8
Histrian	Triestinis	1733	1733	—	—

The mean stature is 1731 ± 1.73 and S.D. 3.6. The two values 1728 and 1733 indicate the limits of the range—5 m.m. The delta-field is 1726—1736 = 10 m.m. The difference between the two means is " non-significant " by t-test. The mean of the S.D. series is 2.5 m.m.

Then I formed up a series with the five provincial values, obtaining a mean stature of 1729 ± 2.64, and a S.D. of 8.8; its range is 1716—1740 = 24 m.m. and its delta-field is 1721—1737 = 16 m.m. Three of these five values fall into the limits of A ± 1 σ, the other two into the field of A ± 2 σ. The mean of the S.D. series is 6.4. The index of provincial variability is 10.8, which is a

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sign of the high dispersion of these values and, parallel, of their centrifugal tendency: the maximum is that of the Gorizianis with a deviation of 11 m.m.

These values show that there is a genetical influence which agrees with our historical sources. This territory was subjected in the high Middle Ages after the fall of the Roman Empire, to a number of invasions and passage of the Barbarians, *i.e.* Scandinavian, Germanic and Slavian military tribes; the Euganean plain constituted an immense door for entering the Padan Valley. Many groups of these Barbarians, particularly the Longbardens, settled down in rural communities, intercrossing or assimilating the sparse Venetis population which lived *ab initio* in this territory.

It is impossible now to identify or to discriminate these few genetical varieties which form this demographical complex that I have, for short, called "Illyrian." Undoubtedly there is a fundamental Venitian stock of the same phenotype of the Euganeans, but it is submerged by the waves of Barbarians which spread over its territory. What we ascertain now is only the fact that the population shows the highest stature and that it is heterogeneous. The provincial Belluneses were surely affected by the invaders' influence, while the other two populations of the Friuli plain show the high degree of the invaders' height. Surely the Triestiners population, being a coastal population, shows a stature which is influenced by environmental and social (being a "chief town" one) factors. At the end of the eastern end of this territory (with Trieste) there are the Gorizianis, the tallest among these populations (1740), a quality to be ascribed to the influence of the Illyrians of the southern Slavian stock.

I calculated then the mean stature of the population of the administrative region of Friuli-Venetia Giulia, regardless of *stirpi*, obtaining a statural mean of 1732 $\pm$ 2.11 and S.D. 6.2.

(*To be continued*)

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Book Reviews

GENETICS AND SOCIETY

Edited by Jack B. Bresler. Addison-Wesley Publishing Company, in the Life Science Series, Reading, Mass., Menlo Park, Cal., London and Don Mills, Ontario. 1973. Pp. 281. Price $4.50.

This collection of readings, the jacket blurb asserts, "represents one of the first attempts to introduce students to the newly emerging, rapidly developing field of *socio-genetics*, as distinct from categories such as Mendelian, evolutionary, biochemical and physiological." The 21 papers selected by editor Jack Bresler were chosen on the basis of brevity, recency of publication and lack of refined mathematics beyond the grasp of college students with a first-year course in genetics and biology behind them.

The symposium seems to this reviewer admirable in several respects. The collection is singularly free of the piety toward environmentalist explanations of social phenomena which is still both rife and fashionable in the American academic world. The articles are, to an unusual degree in a college-level collection, original contributions which shed new light on some of the interrelationships subsisting between the advancing science of genetics and institutional, reproductive, I.Q.-fertility, hybridization-fertility and other related patterns. For the most part, they are culled from such outstanding periodicals as *Science, Nature, Journal of Heredity, Lancet* and *Perspectives in Biology and Medicine*.

John H. Heller's article on "Human Chromosome Abnormalities as Related to Physical and Mental Dysfunction" is a valuable summary of what is currently known concerning the various chromosomal polyploidies and aneuploidies such as Down's syndrome, cri-du-chat, the Turner and Klinefelder syndromes and the XYY males. Total occurrence of these genetic faults in the U.S. population of 202 million is placed at 1.1 million (1969). Since Klinefelder and Down syndromes are strongly positively correlated with age of mother and since both produce mental deficiency, postponement of the age of marriage, as in Ireland, may cause increased incidence of amentia.

Brief articles demonstrate that patterns of alcohol and coffee consumption are probably genetically conditioned (analysis of monozygotic twin behavior) and that Chinese-American and Caucasian-American neonates show radical differences in reaction to stimuli, the Chinese-American infants displaying "relative imperturbability or ready accommodation to external changes." Another brief paper shows conclusively that Welsh fertility is consistently lower than English and that this difference is probably genetically caused. An equally careful investigation of the fact that twin-I.Q.s average about 5 points below those of singletons and triplet-I.Q.s some 10 points lower demonstrates that the difference is probably neither genetic nor caused by uterine environment.

In a technical article on "Freeze Preservation of Human Sperm," Behrman and Ackerman make suggestions which seem to this reviewer illuminating. First, a protected sperm bank, containing material from men of outstanding intelligence, health and vigor, would mitigate some of the genetic consequences of possible nuclear holocaust. Second, artificial insemination of women with previously frozen sperm carries with it the possible dangers of natural selection of parental donors on the basis of comparative ability of their sperm to survive the freezing process and gene selection from the same donor on the same basis.

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Editor Jack B. Bresler's article, "Outcrossings in Caucasians and Fetal Loss," originally published in the 1970 issue of *Human Biology*, casts important light on possible consequences of race-crossing in man. Some 708 family histories of women patients in the Providence Lying-in Hospital were taken. Only patients all of whose great grandparents were of the white race were included in the sample. The number of countries where the great grandparents were born was positively correlated in linear fashion with miscarriage and involuntary abortion: ". . . fetal loss (F_1 generation) in matings of the parental generation (P_1) increases cumulatively by approximately 2.5 per cent to 3 per cent with each additional country of birth in the great-grandparental generation (P_3)." A less precise positive correlation was found between percentage of fetal loss and distance in miles between the birthplaces of the parents. In a previous study, Bresler showed that the greater the number of national origins in the background, the smaller was completed mean family size. The explanation, Bresler states, is probably genetic: as larger numbers of Mendelian gene pools are brought together "with concomitant imbalance between loci, fetal loss increases."

The collection contains two articles by this reviewer, "Some Genetic Aspects of Plantation Slavery" and "Some Possible Genetic Implications of Carthaginian Child Sacrifice," both from *Perspectives in Biology and Medicine*.

NATHANIEL WEYL

SMALL IS BEAUTIFUL

By E. F. Schumacher. Blond and Briggs, London. Pp. 288. Price £3.25 in U.K. only.

This book by the economic adviser to the National Coal Board 1950 to 1970, founder-chairman of the Intermediate Technology Development Group and director of the Scott-Bader Institute is a first study of economics as if people mattered. It is therefore of importance to readers of THE MANKIND QUARTERLY.

Western culture is based on economics, a quantitative art of limited value; on science which has no connection with religion or metaphysics; on concepts of growth in a world of finite resources. Since the USA with 5.6 per cent of the world's population uses 40 per cent of the world's primary resources it is obvious that their culture is not for export. Schumacher pours scorn on big fantastic notions of government planners but desiderates a multiplicty of small-scale units. In his view Sicco Mansholt's agricultural plan is the crudest materialism.

He examines the Victorian teachings which led to our amoral cultural pattern, dominated by private enterprise and its growth into huge corporations. He suggests at the end of the book how these companies could be converted to more socially valuable units without sickening loss to individual investors. More importantly he describes how intermediate or appropriate technology can come to the aid not only of millions of villages in the Third World but also in many of the poorer parts of the developed world. Here is a field of endeavour for scientists and technologists who wish to avoid the violence and greed of Western culture as it is.

The present system has appalling genetic consequences. The brightest and most enterprising people leave the country for the cities, which are unable to absorb them. In rural and town areas there evolves an indigenous élite who have lost contact with the ordinary people. Schumacher recognises that only a tiny minority are innovators, but he thinks that a university education should become, not a passport to privilege, but a training for

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service to the people leading in most parts of the world to an agro-industrial culture with a colourful variety of occupations.

The book can be recommended to all who care deeply for enrichment of all the world's native cultures; who seek devolution of centralised government or industrial control; who ask the question what are the natural resources of this area and can we turn them to good account; who see that exports are not an essential ingredient of national happiness; who wish to live in a country "where the art of being a stranger has yet to be imported."

Here we find deep convictions, a religious message of hope—a message far more compelling than the exhortations of politicians trying to operate a no longer workable system. Schumacher's thesis should be built upon. The genetic effects of his recommendations could be thought about more deeply; he does not say enough about rural co-operative systems, and fails to mention the well-proven device of the credit union. Nevertheless there is much food here for thought and more particularly for action.

ROBERT H. S. ROBERTSON

THE INEQUALITY OF MAN

By H. J. Eysenck. Maurice Temple Smith, London, 1973. Pp. 288, 13 figures and 13 diagrams. Price £3.00.

Professor H. J. Eysenck, quite rightly, draws attention to the fact that the notion of the equality of man (as distinct from equality before the law and before God) stems from Rousseau (p. 16). Rousseau was answered in 1890 by Professor T. H. Huxley, even without the knowledge of Mendelism which would have made his arguments more forceful (p. 17). Professor J. B. S. Haldane, despite being a communist, and a world renowned authority on genetics which Mendel had given to science, came to much the same conclusion.

What is of importance is Professor Eysenck's quotation from Huxley: "I have very long entertained the conviction that the revived Rousseauism of our day is working sad mischief, leading astray those who have not the time, even if they possess the ability, to go to the root of the superficially plausible doctrines which are diseminated among them" (p. 18).

Professor Eysenck is undoubtedly right when he says "there is no question whatever that men are created unequal, in the sense that their genes contain the determinants of unequal appearance and development" (p. 23).

The writer makes a plea for *interaction*, the claims of genetics and environment (p. 29).

Professor Eysenck properly points out that even when Jensen wrote his paper in 1969 one could not measure the heritability factor, but that is no longer true; it can be measured (p. 89). The heritability of inheritance is approximately .80 (p. 90).

Ranging widely over numerous statistics and methods of evaluation it seems clear that somewhere between 77 per cent to 88 per cent is due to heredity (p. 102).

Quoting the fact that of the retarded persons (p. 121) in Reed's study 43 per cent never had any children at all, suggests that there may not be an overall fall in intelligence in the nation as a whole. This, however, seems too hard to believe, as all around us we find the drop in standards. It may slow it down, but I do not believe it prevents it.

Eskimo children, brought up in the Arctic, scored as high as the Canadian, and much higher than Negro and Jamaican, which are nearer

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to our culture. "These studies may also serve as additional proof that the oft-repeated criticism that I.Q. tests are made by white, middle-class psychologists for white, middle-class children, and are unfair to children not belonging to this charmed circle, is mistaken. Certainly the makers of the tests employed in these studies did not have Eskimo children in mind when they constructed their test items!" (p. 125).

This is a good book, worthy of careful study, which repudiates effectively all the carefully organised propaganda against the I.Q. tests.

R. GAYRE OF GAYRE

RACE

By John R. Baker. Oxford Press, London, 1974. Pp. 623, 82 illustrations. Price £6.50.

This is a most comprehensive book ranging from Neanderthal man to Hitler, with the definition of the meaning of "species," "race," discussions on the ethnic divisions of race, and criteria of superiority and inferiority. In addition there is a list of appendices, bibliography, index, and table of races and sub-races. On the whole, it does not deal with the political aspects of the subject—and that in itself is a relief.

Dr Baker makes the Cushitic peoples of Ethiopia (which he calls Aethiopids) a hybrid between Europids and Negrids—which they might well be, although, in my own thinking I have been more impressed with an Australoid element than a Negroid, and that Australoid has entered into certain of the races of the Bantu.

The writer is quite right in supposing that the peoples of Genesis X (p. 13) were Europoid (See my book on the subject). He is equally right to quote Acts XVII, 24-26, which is woefully translated in our texts. "God . . . hath made of one blood all nations of men." This, as the writer points out (p. 14) is due to the influence of Erasmus, who inserted the word blood. It seems to be that the obvious rendering of this passage is that God hath made by One (Jesus Christ) for all people to dwell on the earth. It is a theological and not an ethnological point.

Dr Baker (p. 16) makes a corrective to the exaggerated notion of colour when he quotes Northern Africa, Egypt, and India, as "coloured" but of the Europoid stock. That is a point which Professor Ruggles Gates and I have strongly held. However, while this is due to a mutation probably for colour of one pair of alleles, it must be remembered that the Negro has probably four pairs, and, furthermore, many other features which are other than Europoid. Therefore, while we can cavil at the loose use of "colour" for races opposite of white, it can indicate those so utterly distinct that they have little in common with the white race, except their common humanity.

On pp. 33-38 Dr Baker does justice to the ideas of Count Gobineau which have been grossly misunderstood by current propaganda on behalf of anti-Nazi racialism. He was not associated with that current of ideas, and did not give birth to them. He did not, of course, believe in the common notion, of the all-powerful influences of environment—and neither do we.

Charles Henri-Georges Pouchet, in 1808, wrote his work *De la pluralité des races humaines*. In this work he believed that convergent evolution occurred, which ultimately has led to Peters (1937) and Coon (*The Origin of Human Races*), with which I fully concur—the stocks of mankind have arisen from separate strains from the *Homo erectus* period onward (pp. 38-39).

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"The Australids, shown to be primitive by morphological criteria (chapter 16) did not progress on their own initiative beyond the food-gathering status; nor did those classic prototypes of paedomorphosis, the Bushmen or Sanids (chapter 17). A parallel conclusion is forced upon us if we look at the results of cognition and attainment tests carried out on members of various races living under conditions of civilised life (chapter 25 and pp. 498-9). The Mongolids and Europids did best in both of these types of tests; they were followed (at some distance) by the Indianids, and the Negrids were still less successful" (p. 533).

This is in accord with all our experience and knowledge as it is today. This is a good book, vastly learned, and without the knowledge of its contents any anthropologist will be the loser.

R. GAYRE OF GAYRE

BLACK BRITAIN

By Chris Mullard. George Allen and Unwin, 1973. Pp. 194. Price £3.95.

In any other country this would be looked upon as an important book. Certainly, for a small racial minority in India, Pakistan, or any country in Black Africa, to speak as this man does would mean that he would be thrown out of the country at once. He was born in the U.K. 1944. He became active in race relations—which means as agitator—just after Notting Hill riots, and between 1961 and 1965 worked with black communities in North Kensington and Brixton. Later he became (according to his own admissions) full-time regional secretary for the Tyneside Campaign Against Racial Discrimination when he organised the first demonstration of race in May 1968. The astonishing thing is he was made the present community relations officer for Tyneside.

Here is a black man, born in Britain, who alleges it is a thoroughly racist society, and the 1970s, according to him will witness racial civil war and riots. Blacks born in Britain will not tolerate "humiliation." Who humiliates them: everywhere, in advanced societies throughout the world they are at the bottom of the social ladder. They have an average I.Q. lower than White, Yellow, or Red, or Brown. But this is something that no one is responsible for.

This book is certainly a warning—but not as the writer thinks. It is a warning that Britain must do as they do themselves in their own countries. That is to turn out all agitators, and take in only those who will show sufficient gratitude to be here. He, or his parents, were not forced to come to Britain.

Certainly a time will come when the British toleration will come to an end, if this book is to project the course of Black relations in Britain.

R. GAYRE OF GAYRE

HYPOCRISY IN ACADEMIA? A CASE STUDY OF AN AAUP INVESTIGATION.

By Henry L. Ashmore. Vantage Press, New York. Pp. 105. Price $5.95.

This book concerns a certain Professor X, Assistant Professor (lecturer) of Sociology at Armstrong State College, Georgia, who was suspended on full pay pending the outcome of his trial for contributing to the delinquency of minors, by having pornographic literature in his room. The outcome was a hue and cry by the American Association of University Professors

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on his behalf, alleging he was being punished before the courts had found him guilty. (This is a curious allegation as George Pottinger, the Scottish Civil Servant was placed on suspension with full pay pending his trial with Poulson this year. It would seem a normal thing for a college to do pending the outcome of the case).

The special committee appointed to investigate the business found that President Henry L. Ashmore had not abused his position in suspending the appellant. Our sympathies are with Professor Ashmore. The Departments of Sociology are hag-ridden with these attempts to introduce politics into science. The last time we lectured on Zimbabwe at this college the Negro students present stumped out two minutes after the start of the lecture although all that had been outlined to their hearing was that North Africa was part of the Carthaginian and Roman Empires, and was White.

R. GAYRE OF GAYRE

SLAVERY: PAST AND PRESENT

By Roy Pinney. Thomas Nelson Inc., New York, 1972. Pp. 155. Price $4.95.

This book is good and bad in parts. The early part deals with early historic slavery. The second part deals with American, and it is here that one must take issue with the writer, not so much in the facts he gives, but the emphasis in his work. For instance, some Hausa and Fulani, just by chance, must have been enslaved, but they were very few against the whole. Again, to take Herkovitz and cite literally all the littoral races of Africa is wholly wrong. Perhaps a few elements came from the east coast of Africa but they could not have been many. Then, to say that priests and literate Moslems were among them, as though these were a large proportion, and so were more cultured than the slave owners, is wholly misleading. A few may have been enslaved. But on the whole it was those very literate Moslems who were enslaving the black Africans on their own behalf.

Again, while all the facts are quoted for the slavers delivering their slaves in good condition, some old stories are given of horrific voyages. Of course, there were bad passages, and indenture labourers from Europe, and the sailors too suffered from these as much as the slaves. This is not written to condone slavery, but at least to maintain a sense of proportion.

The bad conditions for transporting slaves came after the British and other countries made it illegal, and hunted down the slave ships at sea. As a consequence, these had to be faster vessels, and so less bluff in their proportions. This the author admits (pp. 97 and 98).

The author focuses in the latter part of his book, on slavery today. "Today, slavery has gone underground and, as a result, is cloaked in subterfuge and half-truths" (p. 105). I have, myself, seen slaves in families in Ethiopia, after Haile Selassie has proclaimed the ending of slavery in Ethiopia.

However, the writer's strictures on Portuguese Angola are simply not true, as far as the Portuguese themselves are concerned. He relies on Basil Davidson, which should be enough to disprove its authenticity. To equate contract labour with slavery is nonsense. To say that with the men away from the village the women are left to cultivate the fields alone (p. 113) shows how little the author is familiar with Africa. The man does not work—he hunts, he fights, he kills.

Again, speaking of Nigeria, to equate bride-price with slavery is, once again, nonsense.

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In discussing Rhodesia, to suggest that because Rhodesian Whites earn as much as the African and Asian communities combined, that the latter is "bordering on slavery" is arrant nonsense. The inheriting of widows, in Rhodesia, in tribal law, is not slavery, but social security for these unfortunates.

This is why we call it good and bad in parts.

R. GAYRE OF GAYRE

THE PEOPLE OF AMERICA

By T. D. Stewart. Weidenfeld and Nicolson, London. Pp. 261, 18 illustrations, 34 figures. Price £4.95.

The author, in his preface, tries not to step out of line with modern interpretations of anthropology by citing UNESCO that the differences between human beings may be as great or greater than between races! Of course they may, but in a restricted number of characters. In questions between Caucasoid or Black, for instance, there is an absolute difference between eyes, hair, prognathism, lips, and formation of many other features. Furthermore, when it comes to the mental and psychic questions the averages differ. However, brushing this aside, as it is typical of the times in which we live, when to proclaim racial difference is a sin, we will proceed to look at his book in other respects.

This book attempts to summarise the history of man in America from the standpoint of physical anthropology. From our point of view, the main interest of the book rests in the first part, which deals with the land bridges between Asia and America and which allowed man to enter the latter.

He points out (p. 25) that Hrdlicka, the American anthropologist, found that some of the peoples of Eastern Siberia were Amerindian in appearance. Somewhere about 28,000 years ago, or more, man entered America (p. 28).

Unfortunately the author does not consistently follow the history or prehistory of the Amerindians, but jumps straight to the Columbian settlements. However, he does throw back to earlier periods later on. He thus discusses the coming of the Vikings in 1117 A.D., and later still the earlier prehistory of the continent.

It will be noted that cross-breeding with Europeans has resulted in the Eskimoes being "more mixed and their physical deterioration has increased" (p. 141).

It is my own belief that a proto-Australoid (related to the Asians) entered America from Asia, before the latter would drift, and both were of the O blood group. But the former were long skulled, so some of this element may account for the long skulls found in early Amerindian types.

Despite certain elements of criticism this is a worthwhile book to be read and studied carefully.

R. GAYRE OF GAYRE

BEFORE COLUMBUS: LINKS BETWEEN THE OLD AND ANCIENT AMERICA

By Cyrus H. Gordon. Turnhouse Press, London, 1972. Pp. 224, 44 illustrations. Price £2.75.

Dr Cyrus Gordon argues for the lack of total isolation of America before Columbus, and points to Negroid and Semitic types, in the form of pottery. It brings people across both oceans (p. 35).

In the fourth century B.C. Theopompus, a Greek, mentions an enormous "continent" outside the Old World. Diodorus Siculus in the first century

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B.C. describes an "island" in the ocean many days to the west of Africa. It is significant that the Aztecs had a tradition that civilisation was brought to America by a bearded white person called Quetzalcoatl, the Plumed Serpent. The Maya had the same tradition.

Civilised contacts with Central America seem to have come from Japan (Jomon period), Rome and Arabia (with coins from the fourth to the eighth century respectively found in a wreck off the coast of Venezuela), and the Coimbra map of North America dated 1424 A.D. Admiral Reis produced in 1513 a map which was based on Alexandrian sources which gives much of the coast of America right down to the Antartic.

The author cites convincing evidence for the visiting of Brazil in the sixth century B.C. by way of the circumnavigation of Africa, three years out from Palestine. This is in complete conformity with my views as found in my work on Zimbabwe.

Dr Gordon goes so far as to aver that B.C. 4000 is the period of first contact with the last, and that this lasted on down to the Christian era. He points to the fact that the Egyptians and the Mayans had a year of 360 days.

Thorshish was to be reached by the ancients from either the west coast of Palestine or the Red Sea! This means that Thorshish lay along the Atlantic coast. Actually Dr Gordon thinks that Mexico could have been ancient Thorshish.

The ancient scriptures of the Maya people derive them as coming across the sea, and this must (for several reasons) be the Atlantic and not the Pacific.

The Mesoamericans have a consistent tradition that civilisation was brought by a bearded white man from the east by boat.

Thales of Miletus in B.C. 505 (according to Herodotus, I, 74) was able to predict solar and lunar eclipses. According to Gordon the minimum coverage needed for such predictions is three longitudinal bands of 120°, and if we regard the Near East as the centre, this, towards the west, takes us to Middle America.

The finding of second century Hebrew coins in Kentucky, a Hebrew inscription in the Appalachian Mountains, and the second millennium B.C. Metcalf Stone with old writing in Georgia, are all pointers in the same direction.

Dr Gordon is head of the Department of Studies of Brandeis University, Massachusetts, and has written a worthwhile book which adds something more to this obscure area of investigation.

POPULATION BIOLOGY OF THE ANCIENT EGYPTIANS

Edited by Brothwell and Chiarelli. Academic Press (Inc) Ltd., 1973. Pp. 168. Price £7.00.

The result of a symposium at Turin in 1969, this is a very thorough investigation of the ancient Egyptian population by a host of writers.

J. Lawrence Angel shows that the lower Egyptian shows Eastern Mediterranean traits. In their study of non-metrical variations of the skull, A. C. Berry and R. J. Berry show that the Ancient Egyptian type is much more like a north Indian series, than one of genetic, Negroid, or North European origin.

D. L. Greene in "The Dental Anthropology of Early Egypt and Nubia" shows that there has been remarkable continuity in time and space in the Nile corridor, and little evidence of mass movements of intrusive peoples.

Andrzej Wiercinski sets out the "Problem of Anthroposcopic Variations

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in Ancient Egyptians," and shows that the Pre- and Early dynastic series of crania from Upper and Lower Egypt show them all as basically belonging to the Caucasoid stock and show greatest resemblances to the series from India.

There is a tremendous amount of information in this authoritative work, and the fact that we do not review the other writers is because it is impossible within the space allotted for review.

PALE INK: TWO ANCIENT RECORDS OF CHINESE EXPLORATIONS IN AMERICA

By Henriette Mertz. The Swallow Press, Chicago, 1973. Pp. 175, 44 illustrations, 8 maps. Price $6.98.

This is an account of two Chinese expeditions to America, one in the fifth century A.D., and the other in the twenty-third century B.C.

Evidence for Jomon pottery from Japan B.C. 2500 in Ecuador seems beyond question today. The Chinese are credited with the compass in the time of the Emperor Tz-ing-wang, B.C. 1100.

The author believes that Quetzalcoatl, the white-bearded God of the Mesoamericans, was a Buddhist priest. This is far-fetched, as all rationalisations make him come from, and go back by boat to, the East.

On the other hand, her identification of "the Kingdom of Women" with the Hopi Indians appears to be correct, and if so, brilliantly identified.

Certain ceramics Miss Metz sees as of Chinese origin dating from B.C. 2000 or thereabouts.

The book is well-worth reading, as undoubtedly the authoress makes out a good case for Chinese penetration into Mesoamerica, even if we believe the principal one was Caucasoid from the east rather than the west. The two views are not contradictory.

ELEMENTARY STRUCTURES RECONSIDERED: LEVI-STRAUSS ON KINSHIP

By Francis Korn. Tavistock Publications Ltd., London, 1973. Price £2.90.

Claude Levi-Strauss is the protagonist for one version of social anthropology. The work of Francis Korn is a devastating criticism of Levi-Strauss's kinship systems. She says in concluding the book: "In fact, the outcome of my own experience when dealing with Levi-Strauss's specific cases is that when he departs from the work of his predecessors he is usually mistaken. It is a problem, rather, to account for the renown of a theoretician who is unimpressive as an analyst and whose theories, which are seldom original, are regularly refuted by the facts."

CULTURE AND COGNITION: RULES, MAPS, AND PLANS

Edited by James P. Spradley. Chandler Publishing Co., Interplex Publishing, London, 1973. Pp. 400. Price £4.50.

The book is divided into three parts: "Interdisciplinary Perspectives;" "Ethnoscience;" and "Studies in Culture and Cognition," and each is subdivided into chapters written by various writers.

The essay on "The Language of Spiritualist Churches," by Irving I. Zaretsky, is not necessarily of British Churches. Furthermore, there is a seeking of the questioner for information, which does not occur in Britain —and so it seems to me that Zaretsky has fallen among people where ability in the psychic field may well be questioned.

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With contributions by Kenneth E. Boulding, George A. Miller, Herbert Blumer, Robbins Burling and many others, it deals thoroughly with the subjects under review.

ASIA STUDIES AT HAWAII: TEXTILES OF THE INDONESIAN ARCHIPELAGO

By Garrett and Bronwen Solyon. University Press, Hawaii, November 1973. Pp. 100, 49 plates. Price $8.00.

The intention of the book is to provide an enduring record of an exhibition of Indonesian textiles held at Spalding House, Honolulu Academy of Art, in 1973. This ambition the book certainly achieves. The wide variety of designs is quite amazing, ranging from men with guns to absolutely formal designs.

THREE AFRICAN TRIBES IN TRANSITION

Vol. I of *Contemporary Change in Traditional Societies*. Edited by Julian H. Steward. American University Publications Group Ltd., London, and Illinois Press, 1973. Pp. 519, maps, 92 Figures. Price £2.00.

The editor deals with "Tanganyika: A Study of An African Society"; Edward H. Winter and T. O. Beidelman with "Tanganyika: The National and Local Levels"; the "Kipsigis of Kenya" is by Robert A. Manners; and the "Anagutu of Nigeria" by Stanley Diamond.

In dealing with slavery, the book points out the magnets exercised by Arab demands from the north and European slaves from the south of Nigeria, with, in between, domestic slaves as well (p. 384).

The author writes as though the European powers, as such, approved of slavery. But slavery had been endemic in Africa, and he admits that this was so with the Fulani (p. 386).

Incidentally, the photographs of the Fulani show their infusion of white blood—or probably black into white—as was the historical sequence, since they were, originally, a white people.

THE TRANSLATION OF CULTURE: ESSAYS TO E. E. EVANS-PRITCHARD

Edited by T. O. Beidelman. Tavistock Publications, London. Pp. 440. Price £2.50.

This book is by way of a *festschrift* and contains essays by John Barnes, Elizabeth Colson, James J. Fox, Kathleen Gough, C. R. Hallpike, David Hicks, Claude Lévi-Strauss, John Middleton, Rodney Needham, Julian Pitt-Rivers, Peter Rigby, Peter Rivière, Eric Ten Raa, Victor W. Turner, T. O. Beidelman, and by E. E. Evans-Pritchard himself.

It is a tribute to Professor Evans-Pritchard on his retirement from the Chair of Social Anthropology at Oxford.

In his paper John Barnes finds that the disjunction by religious behaviour validated by metaphysics and secular behaviour is determined by ethnics has only a limited taxonomic value. In all the great religions there is the historically persistant contrast between Faith and Works (p. 2).

Pitt-Rivers writes on the word "Caste" but in fact he applies it to all kinds of situations, instead of limiting it to its Indian meaning, and thereby distorts its significance. This is like the way the word *clan*, meaning tribe or family, has been universally applied for something quite different. However, he admits, that it should not be forgotten that there is a resemblance between Indian and Caste which goes back to the original word tribe or breed (p. 249).

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ETHNOLOGY
A quarterly journal, University of Pittsburgh, 4200 Fifth Avenue, Pittsburgh 15213, U.S.A. Price $7 a year. Vol. XII. Nos. 2, 3, 4.

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A yearly journal, National Research Council, via Belsito a Posillipo 17, Naples. Price L4.000. Vol. XXVI, 1972, and Vol. XII, No. 4, 1973.

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A quarterly journal published for Galton Foundation by Blackwell Publications, Oxford, England. Price £2.75. Vol. 5, Nos. 3, 4, 1973.

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Quarterly journal of the Institute of Race Relations, London. Price £1.25. Vol. XIV, No. 4; Vol. XV, Nos. 1, 2, 1973.

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A quarterly journal, Center for Migration Studies, 209 Flagg Place, Staten Island, New York 10304. Price $3.50. Vol. 7, Nos. 2, 3, 1973.

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Journal of the National Institute for Personnel Research, South African Council for Scientific and Industrial Research, Johannesburg. Price R3.00 for three issues. Vol. 15, No. 1, 1973.

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